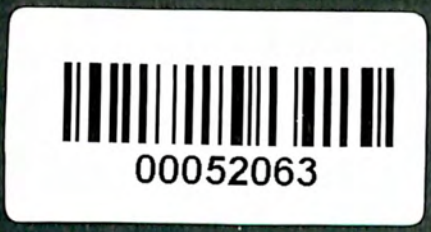
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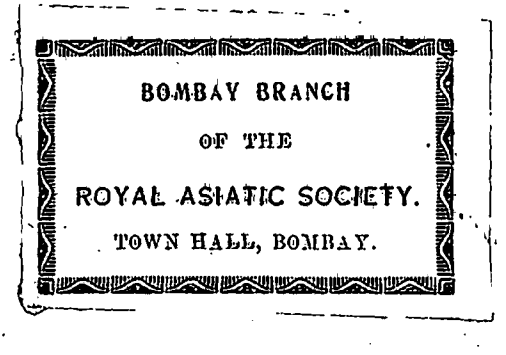

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MISCELLANEOUS WORKS

OF THE LATE

THOMAS YOUNG, M.D., F.R.S., &c.,

AND ONE OF THE EIGHT FOREIGN ASSOCIATES OF THE
NATIONAL INSTITUTE OF FRANCE.

52063
QC

VOLS. I. & II.,

INCLUDING HIS SCIENTIFIC MEMOIRS, &c.,

EDITED BY

GEORGE PEACOCK, D.D.,

F.R.S., F.G.S., F.R.A.S., F.C.P.S., ETC.,

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LOWNDEAN PROFESSOR OF ASTRONOMY IN THE UNIVERSITY OF CAMBRIDGE,
AND FORMERLY FELLOW AND TUTOR OF TRINITY COLLEGE.

Ex of 21

VOLUME I.

LONDON:

JOHN MURRAY, ALBEMARLE STREET.

1855.

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Uniform with Dr. Young's Miscellaneous Works.

This Day, with Portrait, 8vo. 15/,

THE LIFE OF THOMAS YOUNG, M.D., F.R.S.

By GEORGE PEACOCK, D.D., DEAN OF ELY.

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LONDON: PRINTED BY W. CLOWES AND SONS, STAMFORD STREET,
AND CHARING CROSS.

ADVERTISEMENT.

The following edition of the Miscellaneous Scientific Works of Dr. Young contains all his contributions to the Transactions of the Royal Society; the principal Articles prepared for the Supplement of the Encyclopædia Britannica, including a selection of the more elaborate of his Scientific Biographies; many Essays from Nicholson's and Brande's Journals; some Reviews on scientific subjects from the Quarterly Review; and several Essays, either separately published or dispersed in different publications.

An Essay entitled 'Remarks on the Measurement of Minute Particles, especially those of Blood and of Pus,' as illustrating the applications of an optical instrument invented by him called the Eriometer, has been taken from Dr. Young's 'Introduction to Medical Literature.' It was inserted with the view of completing the series of his optical writings, which are contained in the first volume. An Essay 'On the Cohesion of Fluids' is taken from an Appendix to his 'Elementary Illustrations of the Celestial Mechanics of La Place,' with a similar view of completing the series of his Memoirs on this difficult subject. And two sections or chapters from the Mathematical Elements of Natural Philosophy, one of them 'On the Equilibrium and Strength of Elastic Substances,' and the other containing 'Some Propositions on Waves and Sound,' have been inserted, partly on account of their great intrinsic value, and partly because they are not included in the new edition of Dr. Young's Lectures which has been edited by Professor

Kelland. The preceding are the only Articles which formed parts of separate publications of Dr. Young.

It is hardly necessary to observe, that many of his earlier, and not a few of his later, Articles and Essays have been omitted, as being frequently of a controversial nature merely, or as not containing matter which was considered sufficiently new or important to be reprinted; they could not, in fact, have been inserted without very unduly swelling the bulk of this publication.

The Medical Works of Dr. Young, with the exception of a few Reviews, an Essay on Palpitations inserted in the fifth volume of the Medical Transactions of the College of London, and an Essay on Bathing in the Supplement of the Encyclopædia Britannica, are all contained in separate publications. It was at one time proposed to add a volume containing these Essays and some selections from his Medical Works, but the scheme was afterwards abandoned.

A selection from Dr. Young's Optical Correspondence has been subjoined to his Optical Memoirs, as containing materials of no small importance, for illustrating the history of the progress of optical discovery at the memorable period at which they were written.

The notes were added by the Editor, with a view of occasionally illustrating the subjects considered in the text, and of pointing out their bearing upon the researches of other authors. Very careful references have also been generally added, for which the editor is greatly indebted to the assistance of his friend, Mr. Leitch.

CONTENTS OF VOL. I.

Number.		Page
I.	Observations on Vision	1
II.	On the Mechanism of the Eye	12
III.	Outlines of Experiments and Inquiries respecting Sound and Light	64
IV.	An Essay on Cycloidal Curves	99
V.	An Essay on Music	115
VI.	A Letter to Mr. Nicholson, respecting Sound and Light	131
VII.	On the Theory of Light and Colours	140
VIII.	An Account of some Cases of the Production of Colours not hitherto described	170
IX.	Experiments and Calculations relative to Physical Optics	179
X.	Reply to the Animadversions of the Edinburgh Reviewers	192
XI.	Harmonic Sliders	216
XII.	Review of Laplace's Memoir, 'Sur la Loi de la Réfraction extraordinaire dans les Cristaux diaphanes'	220
XIII.	Review of the 'Mémoires de Physique et de Chimie de la Société d'Arcueil'	234
XIV.	Review of Malus, Biot, Seebeck, and Brewster, on Light	260
XV.	The Article 'Chromatics,' from the Supplement to the Encyclopædia Britannica	279
XVI.	Remarks on the Measurement of Minute Particles, especially those of the Blood and of Pus	343
XVII.	Selections from Correspondence relating to Optical Subjects	359
XVIII.	Theoretical Investigations, intended to illustrate the Phenomena of Polarisation	412
XIX.	An Essay on the Cohesion of Fluids	418
XX.	The Article 'Cohesion,' from the Supplement to the Encyclopædia Britannica	454
XXI.	On the Cohesion of Fluids	485
XXII.	Hydraulic Investigations	491
XXIII.	On the Functions of the Heart and Arteries	511
XXIV.	Remarks on the Employment of Oblique Riders and on other Alterations in the Construction of Ships	535
XXV.	A Numerical Table of Elective Attractions	563
XXVI.	A Review of Sir Humphry Davy's Elements of Chemical Philosophy	575

LIST OF PLATES.

Directions to Binder.

Number.			
I.—Observations on Vision	Figs. 1—3	*to face Page*	11
II.—Mechanism of the Eye	Figs. 4—9		
,, ,, ,,	Figs. 10—18		
,, ,, ,,	Figs. 19	,,	61
,, ,, ,,	Figs. 20—30		
,, ,, ,,	Figs. 31—54		
,, ,, ,,	Figs. 55—60		
III.—Sound and Light	Figs. 61—83		
,, ,,	Figs. 84—88		
,, ,,	Figs. 89—100	,,	96
,, ,,	Figs. 101—112		
,,	Fig. 113		
IV.—Cycloidal Curves	Figs. 114—127	,,	114
VII.—Light and Colours	Figs. 128—131	,,	169
XXIV.—Oblique Riders	Figs. 132—140	,,	562

ERRATA.

Page 72, line 7 from bottom, *for* "as" *read* "us."
,, 125, ,, 2 ,, bottom, *for* "$\frac{3}{15}$" *read* "$\frac{8}{15}$."
,, 158, ,, 8 ,, bottom, *for* "part" *read* "path."
,, 268, ,, 5 ,, bottom, *for* "$\varsigma 2$" *read* "cos. ^{2}."
,, 357, ,, 12 ,, top, *for* "supra" *read* "and supra."
,, 392, ,, 12 ,, top, *for* "au" *read* "en."
,, 422, ,, 2 ,, top, *for* "$4ar$" *read* "$4r$."
,, 486, ,, 9 ,, bottom, *for* "$c\ Mf$" *read* "$c^4\ Mf$."
,, 489, ,, 7 ,, bottom, *for* "r^5R+c^4C" *read* "$r^5R=c^4C$."

No. I.

OBSERVATIONS ON VISION.

From the Philosophical Transactions for 1793, vol. lxxxiii., p. 169.

READ MAY 30, 1793.*

IT is well known that the eye, when not acted upon by any exertion of the mind, conveys a distinct impression of those objects only which are situated at a certain distance from itself; that this distance is different in different persons, and that the eye can, by the volition of the mind, be accommodated to view other objects at a much less distance: but how this accommodation is effected, has long been a matter of dispute, and has not yet been satisfactorily explained. It is equally true, though not commonly observed, that no exertion of the mind can accommodate the eye to view objects at a distance greater than that of indolent vision, as may easily

* This memoir was written by Dr. Young in his twentieth year, and led to his election as a Fellow of the Royal Society in the year following. It excited more attention at the time of its publication than its intrinsic importance probably deserved, chiefly in consequence of the discovery announced in it, of the muscularity of the crystalline lens, being claimed by the celebrated John Hunter, whose lectures he was at that time attending. A charge, in fact, was indirectly insinuated in some quarters, though afterwards fully explained and abandoned, that he had derived it from his teacher. Mr. Hunter died in the following October, before he had completed the researches upon which his views had been originally founded: they were resumed by his successor, Mr., afterwards Sir Everard Home, who made, in conjunction with Mr. Ramsden, a series of experiments recorded in the Philosophical Transactions for 1795, which appeared to Dr. Young to negative his conclusion so decisively, that he formally abandoned it, in the following words at the end (p. 68) of his Göttingen Dissertation "De Corporis humani Viribus conservatricibus," published in 1796: "*Sententia nuper de lentis crystallinæ usu in oculo ad diversas rerum videndarum distantias accommodando proposita, neque nova erat neque vera videtur.*" He reiterated the same opinion at the end of his memoir in the Philosophical Transactions, read 16th Nov., 1800, entitled "Outlines of Experiments and Inquiries respecting Sound and Light," which forms No. III. of this volume. The circumstances and experiments which induced him to resume his former views are recorded in the "Memoir on the Mechanism of the Eye," which immediately succeeds the one given in the text, as well as in his reply to the animadversions of the Edinburgh Reviewers, which appears in a subsequent part, No. X., of this volume.—*Note by the Editor.*

be experienced by any person to whom this distance of indolent vision is less than infinite.

The principal parts of the eye, and of its appurtenances, have been described by various authors. Winslow is generally very accurate; but Albinus, in Musschenbroek's *Introductio*, has represented several particulars more correctly. I shall suppose their account complete, except where I mention or delineate the contrary.

The first theory that I find of the accommodation of the eye is Kepler's. He supposes the ciliary processes to contract the diameter of the eye, and lengthen its axis, by a muscular power. But the ciliary processes neither appear to contain any muscular fibres, nor have they any attachment by which they can be capable of performing this action.

Descartes imagined the same contraction and elongation to be effected by a muscularity of the crystalline, of which he supposed the ciliary processes to be the tendons. He did not attempt to demonstrate this muscularity, nor did he enough consider the connection with the ciliary processes. He says, that the lens in the mean time becomes more convex, but attributes very little to this circumstance.

De la Hire maintains that the eye undergoes no change, except the contraction and dilatation of the pupil. He does not attempt to confirm this opinion by mathematical demonstration; he solely rests it on an experiment which has been shown by Dr. Smith to be fallacious. Haller too has adopted this opinion, however inconsistent it seems with the known principles of optics, and with the slightest regard to hourly experience.

Dr. Pemberton supposes the crystalline to contain muscular fibres, by which one of its surfaces is flattened while the other is made more convex. But, besides that he has demonstrated no such fibres, Dr. Jurin has proved that a change like this is inadequate to the effect.

Dr. Porterfield conceives that the ciliary processes draw forward the crystalline, and make the cornea more convex. The ciliary processes are, from their structure, attachment, and direction, utterly incapable of this action; and by Dr. Jurin's calculations, there is not room for a sufficient motion of this

kind, without a very visible increase in the length of the eye's axis: such an increase we cannot observe.

Dr. Jurin's hypothesis is, that the uvea, at its attachment to the cornea, is muscular, and that the contraction of this ring makes the cornea more convex. He says, that the fibres of this muscle may as well escape our observation, as those of the muscle of the interior ring. But if such a muscle existed, it must, to overcome the resistance of the coats, be far stronger than that which is only destined to the uvea itself; and the uvea, at this part, exhibits nothing but radiated fibres, losing themselves, before the circle of adherence to the sclerotica, in a brownish granulated substance, not unlike in appearance to capsular ligament, common to the uvea and ciliary processes, but which may be traced separately from them both. Now at the interior ring of the uvea, the appearance is not absolutely inconsistent with an annular muscle. His theory of accommodation to distant objects is ingenious, but no such accommodation takes place.

Musschenbroek conjectures that the relaxation of the ciliary zone, which appears to be nothing but the capsule of the vitreous humour where it receives the impression of the ciliary processes, permits the coats of the eye to push forwards the crystalline and cornea. Such a voluntary relaxation is wholly without example in the animal economy, and were it to take place, the coats of the eye would not act as he imagines, nor could they so act unobserved. The contraction of the ciliary zone is equally inadequate and unnecessary.

Some have supposed the pressure of the external muscles, especially the two oblique muscles, to elongate the axis of the eye.* But their action would not be sufficiently regular, nor sufficiently strong; for when a much greater pressure is made on the eye, than they can be supposed capable of effecting, no sensible difference is produced in the distinctness of vision.

* Dr. Hosack (Phil. Trans., 1794, vol. lxxxiv. p. 196) has asserted that external pressure can alter the focus of the eye, but I have applied it under the most favourable circumstances without this effect. Mr. Ramsden's microscopical observations have brought new arguments of great strength in favour of this opinion.—*MS. Note by Dr. Young.*

Others say that the muscles shorten the axis: these have still less reason on their side.

Those who maintain that the ciliary processes flatten the crystalline, are ignorant of their structure, and of the effect required: these processes are yet more incapable of drawing back the crystalline, and such an action is equally inconsistent with observation.

Probably other suppositions may have been formed, liable to as strong objections as those opinions which I have enumerated.

From these considerations, and from the observation of Dr. Porterfield and others, that those who have been couched have no longer the power of accommodating the eye to different distances, I had concluded that the rays of light, emitted by objects at a small distance, could only be brought to foci on the retina by a nearer approach of the crystalline to a spherical form; and I could imagine no other power capable of producing this change than a muscularity of a part, or the whole, of its capsule.*

But in closely examining with the naked eye, in a strong light, the crystalline from an ox, turned out of its capsule, I discovered a structure which appears to remove all the difficulties with which this branch of optics has long been obscured. On viewing it with a magnifier, this structure became more evident.

The crystalline lens of the ox is an orbicular, convex, transparent body, composed of a considerable number of similar coats, of which the exterior closely adhere to the interior. Each of these coats consists of six muscles, intermixed with a gelatinous substance, and attached to six membranous tendons. Three of the tendons are anterior, three posterior; their length is about two-thirds of the semi-diameter of the coat; their arrangement is that of three equal and equidistant rays, meeting in the axis of the crystalline; one of the anterior is directed towards the outer angle of the eye, and one of the posterior

* The late experiments of Mr. Home and Mr. Ramsden (Phil. Trans. 1795, vol. lxxxv. p. 453) have so far controverted this received opinion, that the whole theory of the muscularity of the crystalline lens now stands on very weak foundations.—*MS. Note by Dr. Young.*

towards the inner angle, so that the posterior are placed opposite to the middle of the interstices of the anterior; and planes passing through each of the six, and through the axis, would mark on either surface six regular equidistant rays. The muscular fibres arise from both sides of each tendon; they diverge till they reach the greatest circumference of the coat, and, having passed it, they again converge, till they are attached respectively to the sides of the nearest tendons of the opposite surface. The anterior or posterior portion of the six viewed together exhibits the appearance of three penniformi-radiated muscles. The anterior tendons of all the coats are situated in the same planes, and the posterior ones in the continuations of these planes beyond the axis. Such an arrangement of fibres can be accounted for on no other supposition than that of muscularity. This mass is inclosed in a strong membranous capsule, to which it is loosely connected by minute vessels and nerves; and the connection is more observable near its greatest circumference. Between the mass and its capsule is found a considerable quantity of an aqueous fluid, the liquid of the crystalline.

I conceive, therefore, that when the will is exerted to view an object at a small distance, the influence of the mind is conveyed through the lenticular ganglion, formed from branches of the third and fifth pairs of nerves, by the filaments perforating the sclerotica, to the orbiculus ciliaris, which may be considered as an annular plexus of nerves and vessels; and thence by the ciliary processes to the muscle of the crystalline, which, by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina. The disposition of fibres in each coat is admirably adapted to produce this change; for, since the least surface that can contain a given bulk is that of a sphere (Simpson's Fluxions, p. 486), the contraction of any surface must bring its contents nearer to a spherical form. The liquid of the crystalline seems to serve as a synovia in facilitating the motion, and to admit a sufficient change of the muscular part, with a smaller motion of the capsule.

It remains to be inquired whether these fibres can produce

an alteration in the form of the lens sufficiently great to account for the known effects.

In the ox's eye, the diameter of the crystalline is 700 thousandths of an inch, the axis of its anterior segment 225, of its posterior 350. In the atmosphere it collects parallel rays at the distance of 235 thousandths. From these data we find, by means of Smith's Optics, Art. 366, and a quadratic, that its ratio of refraction is as 10000 to 6574.* Hauksbee makes it only as 10000 to 6832,7, but we cannot depend on his experiment, since he says that the image of the candle which he viewed was enlarged and distorted; a circumstance that he does not explain, but which was evidently occasioned by the greater density of the central parts. Supposing, with Hauksbee and others, the refraction of the aqueous and vitreous humours equal to that of water, viz. as 10000 to 7465, the ratio of refraction of the crystalline in the eye will be as 10000 to 8806, and it would collect parallel rays at the distance of 1226 thousandths of an inch: but the distance of the retina from the crystalline is 550 thousandths, and that of the anterior surface of the cornea 250; hence (by Smith, Art. 367), the focal distance of the cornea and aqueous humour alone must be 2329. Now, supposing the crystalline to assume a spherical form, its diameter will be 642 thousandths, and its focal distance in the eye 926. Then disregarding the thickness of the cornea, we find (by Smith, Art. 370), that such an eye will collect those rays on the retina, which diverge from a point at the distance of 12 inches and 8 tenths. This is a greater change than is necessary for an ox's eye, for if it be supposed capable of distinct vision at a distance somewhat less than 12 inches, yet it probably is far short of being able to collect parallel rays. The human crystalline is susceptible of a much greater change of form.

The ciliary zone may admit of as much extension as this diminution of the diameter of the crystalline will require; and

* Without doubt this refractive power is greater than the truth on account of the much less density of the superficial parts, by means of which the efficient part is smaller than the measure here stated, which was taken from the outside of the capsule. The conclusion of the calculation is not however materially affected by this difference.—*MS. Note by Dr. Young.*

its elasticity will assist the cellular texture of the vitreous humour, and perhaps the gelatinous part of the crystalline, in restoring the indolent form.*

It may be questioned whether the retina takes any part in supplying the lens with nerves; but, from the analogy of the olfactory and auditory nerves, it seems more reasonable to suppose that the optic nerve serves no other purpose than that of conveying sensation to the brain.

Although a strong light and close examination are required, in order to see the fibres of the crystalline in its entire state, yet their direction may be demonstrated, and their attachment shown, without much difficulty. In a dead eye the tendons are discernible through the capsule, and sometimes the anterior ones even through the cornea and aqueous humour. When the crystalline falls, it very frequently separates as far as the centre into three portions, each having a tendon in its middle. If it be carefully stripped of its capsule, and the smart blast of a fine blow-pipe be applied close to its surface in different parts, it will be found to crack exactly in the direction of the fibres above described, and all these cracks will be stopped as soon as they reach either of the tendons. The application of a little ink to the crystalline is of great use in showing the course of the fibres.

When first I observed the structure of the crystalline, I was not aware that its muscularity had ever been suspected. We have, however, seen that Descartes supposed it to be of this nature; but he seems to think that the accommodation of the eye to a small distance is principally performed by the elongation of the eye's axis. Indeed, as a bell shakes a steeple, so must the coats of the eye be affected by any change in the crystalline; but the effect of this will be very inconsiderable; yet, as far as it does take place, it will co-operate with the other change.

* It has been objected that the crystalline will not reassume its flat form without an antagonistic muscle, but the contrary is demonstrable by taking it out in its capsule and squeezing it between the fingers; for being let go it restores itself. Another experiment may be adduced: close the teeth gently, then make a strong effort to bite; a considerable swell of the masseter muscle may be externally felt without any nearer approach of the jaws to each other: this swelling subsides immediately as the mind relaxes the muscle. No doubt the cross connection of cellular membrane acts in these cases, and Zinn has described cross threads even in the crystalline.—*MS. Note by Dr. Young.*

But the laborious and accurate Leeuwenhoek, by the help of his powerful microscopes, has described the course of the fibres of the crystalline, in a variety of animals; and he has even gone so far as to call it a muscle;* but no one has pursued the hint, and probably for this reason, that from examining only dried preparations, he has imagined that each coat consists of circumvolutions of a single fibre, and has entirely overlooked the attachment of the fibres to tendons; and if the fibres were continued into each other in the manner that he describes, the strict analogy to muscle would be lost, and their contraction could not, conveniently, have that effect on the figure of the lens, which is produced by help of the tendons. Yet notwithstanding neither he, nor any other physiologist, has attempted to explain the accommodation of the eye to different distances by means of these fibres, still much anatomical merit must be allowed to the faithful description, and elegant delineation, of the crystallines of various animals, which he has given in the Philosophical Transactions, Vol. XIV. p. 780, and Vol. XXIV. p. 1723. It appears, from his descriptions and figures, that the crystalline of hogs, dogs, and cats, resembles what I have observed in oxen, sheep, and horses; that in hares and rabbits the tendons on each side are only two, meeting in a straight line in the axis; and that in whales they are five, radiated in the same manner as where there are three. It is evident that this variety will make no material difference in the action of the muscle. I have not yet had an opportunity of examining the human crystalline, but from its readily dividing into three parts, we may infer that it is similar to that of the ox. The crystalline in fishes† being spherical, such a change as I attribute to the lens in quadrupeds cannot take place in that class of animals.

It has been observed that the central part of the crystalline becomes rigid by age, and this is sufficient to account for

* "Now if the crystalline humour (which I have sometimes called the crist. muscle) in our eyes," &c. Phil. Trans. vol. xxiv. p. 1729.—"*Crystallinum musculum*, alias *humorem crystallinum dictum*," &c. Leeuwenh. op. omn. I. p. 102.

† The lens of fishes may be easily understood by considering it as of the nature of ligament, though more horny, for whatever Hunter has said of the cuttle-fish, I cannot conceive what could be the action of such a spherical muscle.—*MS. Note by Dr. Young.*

presbyopia, without any diminution of the humours; although I do not deny the existence of this diminution, as a concomitant circumstance.

I shall here beg leave to attempt the solution of some optical queries, which have not been much considered by authors.

1. Musschenbroek asks, What is the cause of the lateral radiations which seem to adhere to a candle viewed with winking eyes? I answer, the most conspicuous radiations are those which, diverging from below, form, each with a vertical line, an angle of about seven degrees; this angle is equal to that which the edges of the eyelids when closed make with a horizontal line; and the radiations are evidently caused by the reflection of light from those flattened edges. The lateral radiations are produced by the light reflected from the edges of the lateral parts of the pupillary margin of the uvea, while its superior and inferior portions are covered by the eyelids. The whole uvea being hidden before the total close of the eyelids, these horizontal radiations vanish before the perpendicular ones.

2. Some have inquired, Whence arises that luminous cross, which seems to proceed from the image of a candle in a looking-glass? This is produced by the direction of the friction by which the glass is commonly polished: the scratches placed in a horizontal direction, exhibiting the perpendicular part of the cross, and the vertical scratches the horizontal part, in a manner that may easily be conceived.

3. Why do sparks appear to be emitted when the eye is rubbed or compressed in the dark? This is Musschenbroek's fourth query. When a broadish pressure, as that of the finger, is made on the opaque part of the eye in the dark, an orbicular spectrum appears on the part opposite to that which is pressed: the light of the disc is faint, that of the circumference much stronger; but when a narrow surface is applied, as that of a pin's head, or of the nail, the image is narrow and bright. This is evidently occasioned by the irritation of the retina at the part touched, referred by the mind to the place from whence light coming through the pupil would fall on this spot; the

irritation is greatest where the flexure is greatest, *viz.* at the circumference, and sometimes at the centre, of the depressed part. But in the presence of light, whether the eye be open or closed, the circumference only will be luminous, and the disc dark; and if the eye be viewing any object at the part where the image appears, that object will be totally invisible. Hence it follows, that the tension and compression of the retina destroy all the irritation, except that which is produced by its flexure; and this is so slight on the disc, that the apparent light there is fainter than that of the rays arriving at all other parts through the eyelids. This experiment demonstrates a truth, which may be inferred from many other arguments, *viz.*, that the supposed rectification of the inverted image on the retina does not depend on the direction of the incident rays. Newton, in his sixteenth query, has described this phantom as of pavonian colours, but I can distinguish no other than white; and it seems most natural that this, being the compound or average of all existing sensations of light, should be produced when nothing determines to any particular colour. This average seems to resemble the middle form, which Sir Joshua Reynolds has elegantly insisted on in his discourses; so that perhaps some principles of beautiful contrast of colours may be drawn from hence, it being probable that those colours which together approach near to white light will have the most pleasing effect in apposition. It must be observed, that the sensation of light from pressure of the eye subsides almost instantly after the motion of pressure has ceased, so that the cause of the irritation of the retina is a change, and not a difference, of form; and therefore the sensation of light appears to depend immediately on a minute motion of some part of the optic nerve.

If the anterior part of the eye be repeatedly pressed so as to occasion some degree of pain, and a continued pressure be then made on the sclerotica, while an interrupted pressure is made on the cornea; we shall frequently be able to observe an appearance of luminous lines, branched, and somewhat connected with each other, darting from every part of the field of view, towards a centre a little exterior and superior to the axis of the eye. This centre corresponds to the insertion of the

Fig 1.

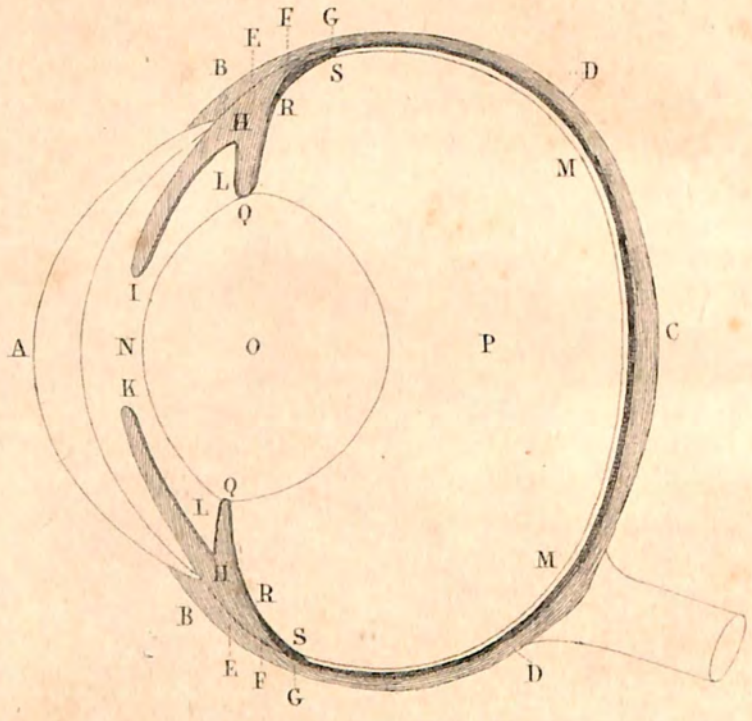


Fig. 2

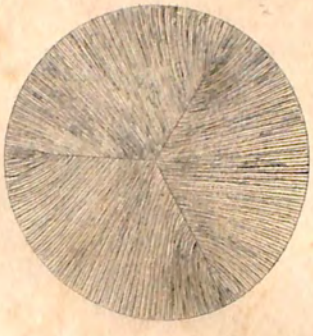
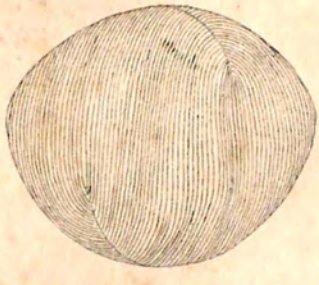


Fig. 3



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optic nerve, and the appearance of lines is probably occasioned by that motion of the retina which is produced by the sudden return of the circulating fluid, into the veins accompanying the ramifications of the arteria centralis, after having been detained by the pressure which is now intermitted. As such an obstruction and such a re-admission must require particular circumstances, in order to be effected in a sensible degree, it may naturally be supposed that this experiment will not always easily succeed.

Explanation of the Figures.

Fig. 1. A vertical section of the ox's eye, of twice the natural size.
A. The cornea, covered by the tunica conjunctiva.
BCB. The sclerotica, covered at BB by the tunica albuginea, and tunica conjunctiva.
DD. The choroid, consisting of two laminas.
EE. The circle of adherence of the choroid and sclerotica.
FG, FG. The orbiculus ciliaris.
HI, HK. The uvea: its anterior surface the iris; its posterior surface lined with pigmentum nigrum.
IK. The pupil.
HL, HL. The ciliary processes, covered with pigmentum nigrum.
MM. The retina.
N. The aqueous humour.
O. The crystalline lens.
P. The vitreous humour.
QR, QR. The zona ciliaris.
RS, RS. The annulus mucosus.
Fig. 2. The structure of the crystalline lens, as viewed in front.
Fig. 3. A side view of the crystalline.

No. II.

ON THE MECHANISM OF THE EYE.

From the Philosophical Transactions for 1801, vol. xcii. p. 23.

Read November 27, 1800.

I.—In the year 1793, I had the honour of laying before the Royal Society, some observations on the faculty by which the eye accommodates itself to the perception of objects at different distances.* The opinion which I then entertained, although it had never been placed exactly in the same light, was neither so new, nor so much forgotten, as was supposed by myself, and by most of those with whom I had any intercourse on the subject. Mr. Hunter, who had long before formed a similar opinion, was still less aware of having been anticipated in it, and was engaged, at the time of his death, in an investigation of the facts relative to it; † an investigation for which, as far as physiology was concerned, he was undoubtedly well qualified. Mr. Home, with the assistance of Mr. Ramsden, whose recent loss this Society cannot but lament, continued the inquiry which Mr. Hunter had begun; and the results of his experiments appeared very satisfactorily to confute the hypothesis of the muscularity of the crystalline lens. ‡ I therefore thought it incumbent on me to take the earliest opportunity of testifying my persuasion of the justice of Mr. Home's conclusions, which I accordingly mentioned in a Dissertation published at Göttingen in 1796, § and also in an Essay presented last year to this Society. ‖ About three months ago, I was induced to resume

* Supra, No. I.

† Phil. Trans. for 1794, p. 21.

‡ Phil. Trans. for 1795, p. 1.

§ De Corporis humani Viribus conservatricibus, p. 68.

‖ Infra, No. III. p. 96.

the subject, by perusing Dr. Porterfield's paper on the internal motions of the eye;* and I have very unexpectedly made some observations, which I think I may venture to say, appear to be finally conclusive in favour of my former opinion, as far as that opinion attributed to the lens a power of changing its figure. At the same time, I must remark, that every person who has been engaged in experiments of this nature, will be aware of the extreme delicacy and precaution requisite, both in conducting them, and in drawing inferences from them; and will also readily allow, that no apology is necessary for the fallacies which have misled many others, as well as myself, in the application of those experiments to optical and physiological determinations.

II.—Besides the inquiry respecting the accommodation of the eye to different distances, I shall have occasion to notice some other particulars relative to its functions; and I shall begin with a general consideration of the sense of vision. I shall then enumerate some dioptrical propositions subservient to my purposes, and describe an instrument for readily ascertaining the focal distance of the eye. On these foundations I shall investigate the dimensions and refractive powers of the human eye in its quiescent state; and the form and magnitude of the picture which is delineated on the retina. I shall next inquire, how great are the changes which the eye admits, and what degree of alteration in its proportions will be necessary for these changes, on the various suppositions that are principally deserving of comparison. I shall proceed to relate a variety of experiments which appear to be the most proper to decide on the truth of each of these suppositions, and to examine such arguments as have been brought forwards, against the opinion which I shall endeavour to maintain; and I shall conclude with some anatomical illustrations of the capacity of the organs of various classes of animals, for the functions attributed to them.

III.—Of all the external senses, the eye is generally supposed to be by far the best understood; yet so complicated and so

* Edinb. Med. Essays, vol. iv. p. 124.

diversified are its powers, that many of them have been hitherto uninvestigated; and on others, much laborious research has been spent in vain. It cannot indeed be denied, that we are capable of explaining the use and operation of its different parts, in a far more satisfactory and interesting manner than those of the ear, which is the only organ that can be strictly compared with it; since, in smelling, tasting, and feeling, the objects to be examined come almost unprepared into immediate contact with the extremities of the nerves; and the only difficulty is, in conceiving the nature of the effect produced by them, and its communication to the sensorium. But the eye and the ear are merely preparatory organs, calculated for transmitting the impressions of light and sound to the retina, and to the termination of the soft auditory nerve. In the eye, light is conveyed to the retina, without any change of the nature of its propagation: in the ear, it is very probable, that instead of the successive motion of different parts of the same elastic medium, the small bones transmit the vibrations of sound, as passive inelastic hard bodies, obeying the motions of the air in their whole extent at the same instant. In the eye, we judge very precisely of the direction of light, from the part of the retina on which it impinges; in the ear, we have no other criterion than the slight difference of motion in the small bones, according to the part of the tympanum on which the sound, concentrated by different reflections, first strikes; hence, the idea of direction is necessarily very indistinct, and there is no reason to suppose, that different parts of the auditory nerve are exclusively affected by sounds in different directions. Each sensitive point of the retina is capable of receiving distinct impressions, as well of the colour as of the strength of light; but it is not absolutely certain, that every part of the auditory nerve is capable of receiving the impression of each of the much greater diversity of tones that we can distinguish; although it is extremely probable that all the different parts of the surface exposed to the fluid of the vestibule, are more or less affected by every sound, but in different degrees and succession, according to the direction and quality of the vibration. Whether or no, strictly speaking, we can hear two sounds, or see two objects,

in the same instant, cannot easily be determined: but it is sufficient that we can do both, without the intervention of any interval of time perceptible to the mind; and indeed we could form no idea of magnitude, without a comparative and therefore nearly contemporary perception of two or more parts of the same object. The extent of the field of perfect vision for each position of the eye is certainly not very great; but it will appear hereafter, that its refractive powers are calculated to take in a moderately distinct view of a whole hemisphere: the sense of hearing is equally perfect in almost every direction.

IV.—Dioptrical Propositions.

Proposition I.—*Phenomenon.*

In all refractions, the ratio of the sine of the angle of incidence to the sine of the angle of refraction is constant. (Newton's Opt. I. Ax. 5; Smith's Opt. 13; Wood's Opt. 24.)

Scholium 1. We shall call it the ratio of m to $m \mp 1$, and $m \mp 1$, n. In refractions out of air into water, $m = 4$ and $n = 3$, very nearly; out of air into glass, the ratio is nearly that of 3 to 2.

Scholium 2. According to Barrow (*Lect. Opt.* ii. 4), Huygens, Euler (*Conject. phys. circa prop. soni et luminis. Opusc. t. ii.*), and the opinion which I lately submitted to the Royal Society,* the velocity of light is the greater the rarer the medium: according to Newton (Schol. Prop. 96. l. i. Princip. Prop. 10. p. 3. l. ii. Opt.), and the doctrine more generally received, the reverse. On both suppositions, it is always the same in the same medium, and varies in the ratio of the sines of the angles. This circumstance is of use in facilitating the computation of some very complicated refractions.

Proposition II.—*Phenomenon.*

If between two refracting mediums, a third medium, terminated by parallel surfaces, be interposed, the whole refraction will remain unchanged. (Newton's Opt. l. i. p. 2. Prop. 3; Smith r. 399; Wood, 105.)

* See p. 81 of this volume.

Corollary. Hence, when the refractions out of two mediums into a third are given, the refraction at the common surface of these mediums may be thus found. Let the refractions given be as $m : n$, and as $m^1 : n^1$; then the ratio sought will be that of $m n^1 : m^1 n$. For instance, let the three mediums be glass, water, and air; then $m = 3$, $n = 2$, $m^1 = 4$, $n^1 = 3$, $m n^1 = 9$, and $m^1 n = 8$. If the ratios be 4 : 3, and 13 : 14, we have $m n^1 : m^1 n :: 39 : 56$; and dividing by $56 - 39$ we obtain 2.3 and 3.3 nearly for m and $m + 1$, in Schol. 1. Prop. 1.

PROPOSITION III.—*Problem.* (Fig. 4.)

At the vertex of a given triangle (CBA), to place a given refracting surface (B), so that the incident and refracted rays may coincide with the sides of the triangle (AB and BC).

Let the sides be called d and e, the base being unity; then in the base take, next to d (or AB), a portion (AE) equal to $\frac{n d}{n d + m e}$, or (AD $= $) $\frac{m d}{m d + n e}$; draw a line (EB or DB) to the vertex, and the surface must be perpendicular to this line, whenever the problem is physically possible. When e becomes infinite, and parallel to the base, take $\frac{n d}{m}$ or $\frac{m d}{n}$ next to d, for the intersection of the radius of curvature.

PROPOSITION IV.—*Theorem.* (Fig. 5.)

In oblique refractions at spherical surfaces, the line (AI, KL), joining the conjugate foci (A, I; K, L), passes through the point (G), where a perpendicular from the centre (H) falls on the line (EF), bisecting the chords (BC, BD), cut off from the incident and refracted rays.

Corollary 1. Let t and u be the cosines of incidence and refraction, the radius being 1, and d and e the respective distances of the foci of incident and refracted rays; then $e = \frac{m d u u}{m d u - n d t - n t t}$.

Corollary 2. For a plane surface, $e = \frac{m d u u}{- n t t}$.

Corollary 3. For parallel rays, $d = \infty$, and $e = \frac{m u u}{m u - n t}$.

Scholium 1. It may be observed, that the caustic by refraction stops short at its cusp, not geometrically, but physically, the total reflection interfering.

Corollary 4. Call $\frac{m\,u\,u}{m\,u-n\,t}$, b, and $\frac{n\,t\,t}{m\,u-n\,t}$, c; then $e = \frac{b\,d}{d-c}$, and $e - b = \frac{b\,c}{d-c}$; or, in words, the rectangle contained by the focal lengths of parallel rays, passing and repassing any surface in the same lines, is equal to the rectangle contained by the differences between these lengths and the distances of any conjugate foci.

Corollary 5. For perpendicular rays, $e = \frac{m\,d}{d-n} = m + \frac{m\,n}{d-n}$; or, if the radius be a, $e = \frac{m\,a\,d}{d-n\,a}$; and if d and e be given to find the radius, $a = \frac{d\,e}{m\,d+n\,e}$.

Corollary 6. For rays perpendicular and parallel, $e = m$, or $e = m\,a$.

Corollary 7. For a double convex lens, neglecting the thickness, call the first radius g, the second h, and $e = \frac{n\,d\,g\,h}{dg+dh-ngh}$. Hence $n = \frac{d\,e}{d+e} \cdot \frac{g+h}{g\,h}$; and for parallel rays, $e = \frac{n\,g\,h}{g+h}$, and $n = e \cdot \frac{g+h}{g\,h}$. If $g = h = a$, $e = \frac{n\,a\,d}{2\,d-n\,a}$; and for parallel rays $e = \frac{n\,a}{2}$: calling this principal focal length b, $e = \frac{d}{d-b}$, as in Cor. 4; whence we have the joint focus of two lenses; also, $b = \frac{d\,e}{d+e}$.

Corollary 8. In a sphere, $e = m\,a \cdot \frac{d+a}{2\,d-(m-2)\,a}$, for the distance from the centre, and $b = \frac{m\,a}{2}$.

Scholium 2. In all these cases, if the rays converge, d must be negative. For instance, to find the joint focus of two convex or concave lenses, the expression becomes, $= e\,\frac{b\,d}{b+d}$.

Corollary 9. In Cor. 3, the divisor becomes ultimately constant; and, when the inclination is small, the focus varies as $u\,u$.

Corollary 10. For parallel rays falling obliquely on a double convex, or double concave lens, of inconsiderable thickness, the radius being 1, $e = \frac{n\,t\,t}{2\,(m\,u - n\,t)}$; which varies ultimately as the square of the cosine of incidence, or as $\frac{m+n}{m\,n}\,t + t^2$.

Scholium 3. In the double convex lens, the thickness diminishes the effect of the obliquity near the axis; in the double concave, it increases it.

Scholium 4. No spherical surface, excepting in one particular case, (Wood, 155,) can collect an oblique pencil of rays, even to a physical point. The oblique rays which we have hitherto considered are only such as lie in that section of the pencil which is made by a plane passing through the centre and the radiant point. They continue in this plane notwithstanding the refraction, and therefore will not meet the rays of the collateral sections, till they arrive at the axis. The remark was made by Sir Isaac Newton, and extended by Dr. Smith (Smith r. 493, 494); it appears, however, to have been too little noticed. (Wood, 362.) The geometrical focus thus becomes a line, a circle, an oval, or other figure, according to the form of the pencil, the nature of the surface, and the place of the plane receiving the image. Some of the varieties of the focal image of a cylindrical pencil obliquely refracted are shown in Fig. 31.

Corollary 11. Hence the line joining the remoter conjugate foci will always pass through the centre. The distance of the remoter focus of parallel rays will be expressed by $f = \frac{m}{m\,u - n\,t}$; and the least circle of aberration will be at the distance $\frac{2\,m\,u\,u}{(1+u\,u)\,.\,(m\,u - n\,t)}$, dividing the length of aberration in the ratio of the distance of its limits from the surface. In the case of Cor. 10. $f = \frac{n}{2\,(m\,u - n\,t)}$.

Corollary 12. This proposition extends also to reflected rays; and in that case, the line from the centre passes through the point of incidence.

PROPOSITION V.—*Problem.*

To find the place and magnitude of the image of a small object, after refraction at any number of spherical surfaces.

Construction. (Fig. 6.) From any point (B) in the object (AB), draw lines to (C), the centre of the first surface, and to (D), the focus of parallel rays coming in a contrary direction: from the intersection of the second line (BD) with the tangent (EF) at the vertex, draw a line (EH) parallel to the axis, and it will cut the first line (BC) in (H), the first image of the point (B). Proceed with this image as a new object, and repeat the operation for each surface, and the last point will be in the image required. For calculation, find the place of the image by Cor. 5. Prop. IV., and its magnitude will be to that of the object, as their respective distances from the centre.

Corollary. If a confused image be received on any given plane, it will be necessary, in order to determine its magnitude, to advert to the aperture admitting the rays. If the aperture be supposed to be infinitely small, it may be considered as a radiant point in order to find the direction of the emergent rays.

PROPOSITION VI.—*Problem.*

To determine the law by which the refraction at a spherical surface must vary, so as to collect parallel rays to a perfect focus.

Solution. Let v be the versed sine to the radius 1; then, at each point without the axis, n remaining the same, m must become $\sqrt{mm \pm 2nv}$; and all the rays will be collected in the principal focus.

Corollary. The same law will serve for a double convex lens, in the case of equidistant conjugate foci, substituting n for m.

PROPOSITION VII.—*Problem.*

To find the principal focus of a sphere, or lens, of which the internal parts are more dense than the external.

Solution. In order that the focal distance may be finite, the density of a finite portion about the centre must be equable: call the radius of this portion $\frac{1}{l}$, that of the sphere being unity; let the whole refraction out of the surrounding medium into this central part, be as m to n; take $r = \frac{\log. l}{\log. m - \log. n}$, and let the density be supposed to vary everywhere inversely as the power $\frac{1}{r}$ of the distance from the centre: then the principal focal distance from the centre will be $\frac{r-1}{2} \cdot \frac{m}{nl - m}$. When $r = 1$, it becomes $\frac{1}{2\,(\text{H. L. } m - \text{H. L. } n)}$. For a lens, deduct one fourth of the difference between its axis and the diameter of the sphere of which its surfaces are portions.

Corollary. If the density be supposed to vary suddenly at the surface, m must express the difference of the refractions at the centre and at the surface; and the focal distance, thus determined, must be diminished according to the refraction at the surface.

Proposition VIII.*—*Problem.*

To find the path of a ray of light falling obliquely on a sphere, of a refractive density varying as any power of the distance from the centre.

The refractive density, in the sense of these propositions, varies as the ratio of the sines, and as the velocity of light in the medium. (Schol. 2. Prop. I.) Let the velocity at the distance x be $x^{-\frac{1}{r}}$; then, considering the refractive force as a species of attraction, we have, in Prop. 41. Lib. 1, Princip. $\sqrt{\text{A B F D}} = x^{-\frac{1}{r}}$, $Q = s$, the sine of incidence, the radius being unity, $Z = s\,x^{-1}$, $D\,c = \frac{s}{2\,x\,x\sqrt{x^{-\frac{2}{r}} - s^2\,x^{-2}}} =$

* The enunciation and demonstration of this Proposition, as printed in the Transactions, were erroneous: their correct forms, as printed in the text, were given in Nicholson's Journal for Aug. 1802, p. 262, at the end of "An Answer to Mr. Gough's Essay on the Theory of Compound Sounds."—*Note by the Editor.*

$\frac{1}{2} s x^{r-2} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}\Big|^{-\frac{1}{2}}$, and the fluxion of the area described by the radius $= -\frac{1}{2} s x^{\frac{1}{r}-2} \dot{x} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}\Big|^{-\frac{1}{2}}$. Let the sine of the inclination to the radius at each point be called y; then $y = s x^{\frac{1}{r}-1}$, $\dot{y} = \frac{1-r}{r} s x^{\frac{1}{r}-2} \dot{x}$, and the fluxion of the area $= \frac{r}{2r-2} \dot{y} \cdot \overline{1 - y y}\Big|^{-\frac{1}{2}}$, of which the fluent is $\frac{r}{2r-2}$ Y, y being the sine of the arc Y; and the angle corresponding is $\frac{r}{r-1}$ Y. The value of that angle being found for any two values of x or y, the difference is the intervening angle described by the radius. This angle is therefore always to the difference of the inclinations as r to $r - 1$, and the deviation is to that difference as 1 to $r - 1$.

Corollary. Hence in the passage to the apsis, and the return to the surface, the deviation is always proportionate to the arc cut off by the incident ray produced: therefore such a sphere could never collect parallel rays to any focus, the lateral density being too small towards the surface.

General Scholium. The two first propositions relate to well-known phenomena; the third can hardly be new; the fourth approaches the nearest to Maclaurin's construction, but is far more simple and convenient; the fifth and sixth have no difficulty; the seventh may either be deduced from the eighth, or may be demonstrated independently of it. The one is abridged by a property of logarithms; the other is derived from the laws of centripetal forces, on the supposition of velocities directly as the refractive densities, correcting the series for the place of the apsis, and making the sine of incidence variable, to determine the fluxion of the angle of deviation.

V.—Dr. Porterfield has employed an experiment, first made by Scheiner, to the determination of the focal distance of the eye; and has described, under the name of an optometer,

a very excellent instrument founded on the principle of the phenomenon.* But the apparatus is capable of considerable improvement; and I shall beg leave to describe an optometer, simple in its construction, and equally convenient and accurate in its application.

Let an obstacle be interposed between a radiant point (R, Fig. 7), and any refracting surface, or lens (C D), and let this obstacle be perforated at two points (A and B) only. Let the refracted rays be intercepted by a plane, so as to form an image on it. Then it is evident, that when this plane (EF) passes through the focus of refracted rays, the image formed on it will be a single point. But if the plane be advanced forwards (to GH), or removed backwards (to IK), the small pencils passing through the perforations will no longer meet in a single point, but will fall on two distinct spots of the plane (G, H; I, K); and, in either case, form a double image of the object.

Let us now add two more radiating points, (S and T, Fig. 8,) the one nearer to the lens than the first point, the other more remote; and when the plane which receives the images passes through the focus of rays coming from the first point, the images of the second and third points must both be double (*s s*, *t t*); since the plane (EF) is without the focal distance of rays coming from the furthest point, and within that of rays coming from the nearest. Upon this principle, Dr. Porterfield's optometer was founded.

But, if the three points be supposed to be joined by a line, and this line to be somewhat inclined to the axis of the lens, each point of the line, except the first point (R, Fig. 9), will have a double image; and each pair of images, being contiguous to those of the neighbouring radiant points, will form with them two continued lines, and the images being more widely separated as the point which they represent is further from the first radiant point, the lines (*s t*, *s t*) will converge on each side towards (*r*) the image of this point, and there will intersect each other.

The same happens when we look at any object through two

* Edinb. Med. Essays, vol. iv. p. 185.

pin-holes, within the limits of the pupil. If the object be at the point of perfect vision, the image on the retina will be single; but, in every other case, the image being double, we shall appear to see a double object: and, if we look at a line pointed nearly to the eye, it will appear as two lines, crossing each other in the point of perfect vision. For this purpose, the holes may be converted into slits, which render the images nearly as distinct, at the same time that they admit more light. The number may be increased from two to four, or more, whenever particular investigations render it necessary.

The optometer may be made of a slip of card-paper, or of ivory, about eight inches in length, and one in breadth, divided longitudinally by a black line, which must not be too strong. The end of the card must be cut as is shown in Fig. 10, in order that it may be turned up, and fixed in an inclined position by means of the shoulders: or a detached piece, nearly of this form, may be applied to the optometer, as it is here engraved. A hole about half an inch square must be made in this part; and the sides so cut as to receive a slider of thick paper, with slits of different sizes, from a fortieth to a tenth of an inch in breadth, divided by spaces somewhat broader: so that each observer may choose that which best suits the aperture of his pupil. In order to adapt the instrument to the use of presbyopic eyes, the other end must be furnished with a lens of four inches focal length; and a scale must be made near the line on each side of it, divided from one end into inches, and from the other according to the table here calculated from Cor. 7. Prop. IV., by means of which, not only diverging, but also parallel and converging rays from the lens are referred to their virtual focus. The instrument is easily applicable to the purpose of ascertaining the focal length of spectacles required for myopic or presbyopic eyes. Mr. Cary has been so good as to furnish me with the numbers and focal lengths of the glasses commonly made; and I have calculated the distances at which those numbers must be placed on the scale of the optometer, so that a presbyopic eye may be enabled to see at eight inches distance, by using the glasses of the focal length placed opposite to the

nearest crossing of the lines; and a myopic eye with parallel rays, by using the glasses indicated by the number that stands opposite their furthest crossing. To facilitate the observation, I have also placed these numbers opposite that point which will be the nearest crossing to myopic eyes; but this, upon the arbitrary supposition of an equal capability of change of focus in every eye, which I must confess is often far from the truth. It cannot be expected, that every person, on the first trial, will fix precisely upon that power which best suits the defect of his sight. Few can bring their eyes at pleasure to the state of full action, or of perfect relaxation; and a power two or three degrees lower than that which is thus ascertained, will be found sufficient for ordinary purposes. I have also added to the second table, such numbers as will point out the spectacles necessary for a presbyopic eye, to see at twelve and at eighteen inches respectively: the middle series will perhaps be the most proper for placing the numbers on the scale. The optometer should be applied to each eye; and, at the time of observing, the opposite eye should not be shut, but the instrument should be screened from its view. The place of intersection may be accurately ascertained, by means of an index sliding along the scale.

The optometer is represented in Fig. 11 and 12; and the manner in which the lines appear, in Fig. 13.

Table I. For extending the scale by a lens of 4 inches focus.

4	2.00	11	2.93	30	3.52	200	3.92	—35	4.51	—12	6.00
5	2.22	12	3.00	40	3.64	∞	4.00	—30	4.62	—11	6.29
6	2.40	13	3.06	50	3.70	—200	4.08	—25	4.76	—10	6.67
7	2.55	14	3.11	60	3.75	—100	4.17	—20	5.00	—9.5	6.90
8	2.67	15	3.16	70	3.78	—50	4.35	—15	5.45	—9.0	7.20
9	2.77	20	3.33	80	3.81	—45	4.39	—14	5.60	—8.5	7.56
10	2.86	25	3.45	100	3.85	—40	4.44	—13	5.78	—8.0	8.00

Table II. For placing the numbers indicating the focal length of convex glasses.

Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.
0	8.00	12.00	18.00	20	13.33	30.00	180.00	8	∞	—24.00	—14.40
40	10.00	17.14	32.73	18	14.40	36.00	∞	7	—56.00	—16.80	—11.45
36	10.28	18.00	36.00	16	16.00	48.00	—144.00	6	—24.00	—12.00	— 9.00
30	10.91	20.00	45.00	14	18.67	84.00	— 63.00	5	—13.33	— 8.57	— 5.92
28	11.20	21.00	50.40	12	24.00	∞	— 36.00	4.5	—10.29	— 7.20	— 6.00
26	11.56	22.29	58.50	11	29.33	—132.00	— 28.29	4.0	— 8.00	— 6.00	— 5.14
24	12.00	24.00	72.00	10	40.00	— 60.00	— 22.50	3.5	— 6.22	— 4.94	— 4.34
22	12.77	26.40	99.00	9	72.00	— 36.00	— 18.00	3.0	— 4.80	— 4.00	— 3.6

Table III. For concave glasses.

Number.	Focus and furthest place.	Nearest place.	Number.	Focus and furthest place.	Nearest place.	Number.	Focus and furthest place.	Nearest place.
0	..	4.00	7	8	2.67	14	3.00	1.71
1	24	3.43	8	7	2.54	15	2.75	1.63
2	18	3.27	9	6	2.40	16	2.50	1.54
3	16	3.20	10	5	2.22	17	2.25	1.44
4	12	3.00	11	4.5	2.12	18	2.00	1.33
5	10	2.86	12	4.0	2.00	19	1.75	1.22
6	9	2.77	13	3.5	1 87	20	1.50	1.02

VI.—Being convinced of the advantage of making every observation with as little assistance as possible, I have endeavoured to confine most of my experiments to my own eyes; and I shall, in general, ground my calculations on the supposition of an eye nearly similar to my own. I shall therefore first endeavour to ascertain all its dimensions, and all its faculties.

For measuring the diameters, I fix a small key on each point of a pair of compasses; and I can venture to bring the rings into immediate contact with the sclerotica. The transverse diameter is externally 98 hundredths of an inch.

To find the axis, I turn the eye as much inwards as possible, and press one of the keys close to the sclerotica, at the external angle, till it arrives at the spot where the spectrum formed by its pressure coincides with the direction of the visual axis, and, looking in a glass, I bring the other key to the cornea. The optical axis of the eye, making allowance of three hundredths for the coats, is thus found to be 91 hundredths of an inch, from the external surface of the cornea to the retina. With an eye less prominent, this method might not have succeeded.

The vertical diameter, or rather chord, of the cornea, is 45 hundredths; its versed sine 11 hundredths. To ascertain the versed sine, I looked with the right eye at the image of the left, in a small speculum held close to the nose, while the left eye was so averted that the margin of the cornea appeared as a straight line, and compared the projection of the cornea with the image of a cancellated scale held in a proper direction behind the left eye, and close to the left temple. The horizontal chord of the cornea is nearly 49 hundredths.

Hence the radius of the cornea is 31 hundredths. It may be thought that I assign too great a convexity to the cornea; but I have corrected it by a number of concurrent observations, which will be enumerated hereafter.

The eye being directed towards its image, the projection of the margin of the sclerotica is 22 hundredths from the margin of the cornea, towards the external angle, and 27 towards the internal angle of the eye: so that the cornea has an eccentricity of one fortieth of an inch, with respect to the section of the eye perpendicular to the visual axis.

The aperture of the pupil varies from 27 to 13 hundredths; at least this is its apparent size, which must be somewhat diminished, on account of the magnifying power of the cornea, perhaps to 25 and 12. When dilated, it is nearly as eccentric as the cornea; but, when most contracted, its centre coincides with the reflection of an image from an object held immediately before the eye; and this image very nearly with the centre of the whole apparent margin of the sclerotica: so that the cornea is perpendicularly intersected by the visual axis.

My eye, in a state of relaxation, collects to a focus on the retina, those rays which diverge vertically from an object at the distance of ten inches from the cornea, and the rays which diverge horizontally from an object at seven inches distance. For, if I hold the plane of the optometer vertically, the images of the line appear to cross at ten inches; if horizontally, at seven. The difference is expressed by a focal length of 23 inches. I have never experienced any inconvenience from this imperfection, nor did I ever discover it till I made these experiments; and I believe I can examine minute objects with as much accuracy as most of those whose eyes are differently formed. On mentioning it to Mr. Cary, he informed me, that he had frequently taken notice of a similar circumstance: that many persons were obliged to hold a concave glass obliquely, in order to see with distinctness, counterbalancing, by the inclination of the glass, the too great refractive power of the eye in the direction of that inclination, (Cor. 10, Prop. IV.) and finding but little assistance from spectacles of the same focal length. The difference is not in the cornea, for it exists

when the effect of the cornea is removed by a method to be described hereafter. The cause is, without doubt, the obliquity of the uvea, and of the crystalline lens, which is nearly parallel to it, with respect to the visual axis: this obliquity will appear, from the dimensions already given, to be about 10 degrees. Without entering into a very accurate calculation, the difference observed is found (by the same corollary) to require an inclination of about 13 degrees; and the remaining three degrees may easily be added, by the greater obliquity of the posterior surface of the crystalline opposite the pupil. There would be no difficulty in fixing the glasses of spectacles, or the concave eye-glass of a telescope, in such a position as to remedy the defect.

In order to ascertain the focal distance of the lens, we must assign its probable distance from the cornea. Now the versed sine of the cornea being 11 hundredths, and the uvea being nearly flat, the anterior surface of the lens must probably be somewhat behind the chord of the cornea; but by a very inconsiderable distance, for the uvea has the substance of a thin membrane, and the lens approaches very near to it: we will therefore call this distance 12 hundredths. The axis and proportions of the lens must be estimated by comparison with anatomical observations; since they affect, in a small degree, the determination of its focal distance. M. Petit found the axis almost always about two lines, or 18 hundredths of an inch. The radius of the anterior surface was in the greatest number 3 lines, but oftener more than less. We will suppose mine to be $3\frac{1}{4}$, or nearly $\frac{3}{10}$ of an inch. The radius of the posterior surface was most frequently $2\frac{1}{2}$ lines, or $\frac{2}{5}$ of an inch.* The optical centre will be therefore $\left(\frac{18 \times 30}{30 + 22} = \right)$ about one-tenth of an inch from the anterior surface: hence we have 22 hundredths, for the distance of the centre from the cornea. Now, taking 10 inches as the distance of the radiant point, the focus of the cornea will be 115 hundredths behind the centre of the lens. (Cor. 5, Prop. IV.) But the actual joint focus is $(91 - 22 =)$ 69 behind the centre: hence, disregarding the thickness of the lens, its principal focal distance is 173 hundredths. (Cor. 7,

* Mém. de l'Acad. de Paris, 1730, p. 6. Ed. Amst.

Prop. IV.) For its refractive power in the eye, we have (by Cor. 7, Prop. IV.) n = 13,5, and m = 14,5. Calculating upon this refractive power, with the consideration of the thickness also, we find that it requires a correction, and comes near to the ratio of 14 to 13 for the sines. It is well known that the refractive powers of the humours are equal to that of water; and, that the thickness of the cornea is too equable to produce any effect on the focal distance.

For determining the refractive power of the crystalline lens by a direct experiment, I made use of a method suggested to me by Dr. Wollaston. I found the refractive power of the centre of the recent human crystalline to that of water, as 21 to 20. The difference of this ratio from the ratio of 14 to 13, ascertained from calculation, is probably owing to two circumstances. The first is, that the substance of the lens being in some degree soluble in water, a portion of the aqueous fluid within its capsule penetrates after death, so as somewhat to lessen the density. When dry, the refractive power is little inferior to that of crown glass. The second circumstance is, the unequal density of the lens. The ratio of 14 to 13 is founded on the supposition of an equable density: but the central part being the most dense, the whole acts as a lens of smaller dimensions; and it may be found by Prop. VII. that if the central portion of a sphere be supposed of uniform density, refracting as 21 to 20, to the distance of one half of the radius, and the density of the external parts to decrease gradually, and at the surface to become equal to that of the surrounding medium, the sphere thus constituted will be equal in focal length to a uniform sphere of the same size, with a refraction of 16 to 15 nearly. And the effect will be nearly the same, if the central portion be supposed to be smaller than this, but the density to be somewhat greater at the surface than that of the surrounding medium, or to vary more rapidly externally than internally; or, if a lens of equal mean dimensions, and equal focal length, with the crystalline, be supposed to consist of two segments of the external portion of such a sphere, the refractive density at the centre of this lens must be as 18 to 17. On the whole, it is probable that the refractive power of the centre of the

human crystalline, in its living state, is to that of water nearly as 18 to 17; that the water imbibed after death reduces it to the ratio of 21 to 20; but that, on account of the unequable density of the lens, its effect in the eye is equivalent to a refraction of 14 to 13 for its whole size. Dr. Wollaston has ascertained the refraction out of air, into the centre of the recent crystalline of oxen and sheep, to be nearly as 143 to 100; into the centre of the crystalline of fish, and into the dried crystalline of sheep, as 152 to 100. Hence the refraction of the crystalline of oxen in water should be as 15 to 14: but the human crystalline, when recent, is decidedly less refractive.

These considerations will explain the inconsistency of different observations on the refractive power of the crystalline; and, in particular, how the refraction which I formerly calculated, from measuring the focal length of the lens,* is so much greater than that which is determined by other means. But, for direct experiments, Dr. Wollaston's method is exceedingly accurate.

When I look at a minute lucid point, such as the image of a candle in a small concave speculum, it appears as a radiated star, as a cross, or as an unequal line, and never as a perfect point, unless I apply a concave lens inclined at a proper angle, to correct the unequal refraction of my eye. If I bring the point very near, it spreads into a surface nearly circular, and almost equably illuminated, except some faint lines, nearly in a radiating direction. For this purpose, the best image is a candle, or a small speculum, viewed through a minute lens at some little distance, or seen by reflection in a larger lens. If any pressure has been applied to the eye, such as that of the finger keeping it shut, the sight is often confused for a short time after the removal of the finger, and the image is in this case spotty or curdled. The radiating lines are probably occasioned by some slight inequalities in the surface of the lens, which is very superficially furrowed in the direction of its fibres: the curdled appearance will be explained hereafter. When the point is further removed, the image becomes evidently oval, the vertical diameter being longest, and the lines a little more distinct than

* Supra, p. 6.

before, the light being strongest in the neighbourhood of the centre; but immediately at the centre there is a darker spot, owing to such a slight depression at the vertex as is often observable in examining the lens after death. The situation of the rays is constant, though not regular; the most conspicuous are seven or eight in number; sometimes about twenty fainter ones may be counted. Removing the point a little further, the image becomes a short vertical line; the rays that diverged horizontally being perfectly collected, while the vertical rays are still separate. In the next stage, which is the most perfect focus, the line spreads in the middle, and approaches nearly to a square, with projecting angles, but is marked with some darker lines towards the diagonals. The square then flattens into a rhombus, and the rhombus into a horizontal line unequally bright. At every greater distance, the line lengthens, and acquires also breadth, by radiations shooting out from it, but does not become a uniform surface, the central part remaining always considerably brightest, in consequence of the same flattening of the vertex which before made it fainter. Some of these figures bear a considerable analogy to the images derived from the refraction of oblique rays, (Schol. 4, Prop. IV.) and still more strongly resemble a combination of two of them in opposite directions; so as to leave no doubt, but that both surfaces of the lens are oblique to the visual axis, and co-operate in distorting the focal point. This may also be verified, by observing the image delineated by a common glass lens, when inclined to the incident rays. (See Fig. 31—43.)

The visual axis being fixed in any direction, I can at the same time see a luminous object placed laterally at a considerable distance from it; but in various directions the angle is very different. Upwards it extends to 50 degrees, inwards to 60, downwards to 70, and outwards to 90 degrees. These internal limits of the field of view nearly correspond with the external limits formed by the different parts of the face, when the eye is directed forwards and somewhat downwards, which is its most natural position; although the internal limits are a little more extensive than the external; and both are well calculated for enabling us to perceive the most readily, such

objects as are the most likely to concern us. Dr. Wollaston's eye has a larger field of view, both vertically and horizontally, but nearly in the same proportions, except that it extends further upwards. It is well known, that the retina advances further forwards towards the internal angle of the eye, than towards the external angle; but upwards and downwards its extent is nearly equal, and is indeed every way greater than the limits of the field of view, even if allowance is made for the refraction of the cornea only. The sensible portion seems to coincide more nearly with the painted choroid of quadrupeds: but the whole extent of perfect vision is little more than 10 degrees; or, more strictly speaking, the imperfection begins within a degree or two of the visual axis, and at the distance of 5 or 6 degrees becomes nearly stationary, until, at a still greater distance, vision is wholly extinguished. The imperfection is partly owing to the unavoidable aberration of oblique rays, but principally to the insensibility of the retina: for, if the image of the sun itself be received on a part of the retina remote from the axis, the impression will not be sufficiently strong to form a permanent spectrum; although an object of very moderate brightness will produce this effect when directly viewed. It would probably have been inconsistent with the economy of nature, to bestow a larger share of sensibility on the retina. The optic nerve is at present very large; and the delicacy of the organ renders it, even at present, very susceptible of injury from slight irritation, and very liable to inflammatory affections; and, in order to make the sight so perfect as it is, it was necessary to confine that perfection within narrow limits. The motion of the eye has a range of about 55 degrees in every direction; so that the field of perfect vision, in succession, is by this motion extended to 110 degrees.

But the whole of the retina is of such a form as to receive the most perfect image, on every part of its surface, that the state of each refracted pencil will admit; and the varying density of the crystalline renders that state more capable of delineating such a picture, than any other imaginable contrivance could have done. To illustrate this, I have constructed a diagram, representing the successive images of a distant object filling the whole extent of view, as they would be formed by the succes-

sive refractions of the different surfaces. Taking the scale of my own eye, I am obliged to substitute, for a series of objects at any indefinitely great distance, a circle of 10 inches radius; and it is most convenient to consider only those rays which pass through the anterior vertex of the lens; since the actual centre of each pencil must be in the ray which passes through the centre of the pupil, and the short distance of the vertex of the lens from this point will always tend to correct the unequal refraction of oblique rays. The first curve (Fig. 19) is the image formed by the furthest intersection of rays refracted at the cornea: the second, the image formed by the nearest intersection; the distance between these, shows the degree of confusion in the image; and the third curve, its brightest part. Such must be the form of the image which the cornea tends to delineate in an eye deprived of the crystalline lens; nor can any external remedy properly correct the imperfection of lateral vision. The next three curves show the images formed after the refraction at the anterior surface of the lens, distinguished in the same manner; and the three following, the result of all the successive refractions. The tenth curve is a repetition of the ninth, with a slight correction near the axis, at F, where, from the breadth of the pupil, some perpendicular rays must fall. By comparing this with the eleventh, which is the form of the retina, it will appear that nothing more is wanting for their perfect coincidence, than a moderate diminution of density in the lateral parts of the lens. If the law, by which this density varies, were more accurately ascertained, its effect on the image might be estimated by means of the eighth proposition; and probably the image, thus corrected, would approach very nearly to the form of the twelfth curve.

To find the place of the entrance of the optic nerve, I fix two candles at ten inches distance, retire sixteen feet, and direct my eye to a point four feet to the right or left of the middle of the space between them: they are then lost in a confused spot of light; but any inclination of the eye brings one or the other of them into the field of view. In Bernoulli's eye, a greater deviation was required for the direction of the axis;* and the

* Comm. Petrop. I. p. 314.

obscured part appeared to be of greater extent. From the experiment here related, the distance of the centre of the optic nerve from the visual axis is found (by Prop. V.) to be 16 hundredths of an inch; and the diameter of the most insensible part of the retina, one-thirtieth of an inch. In order to ascertain the distance of the optic nerve from the point opposite to the pupil, I took the sclerotica of the human eye, divided it into segments, from the centre of the cornea towards the optic nerve, and extended it on a plane. I then measured the longest and shortest distances from the cornea to the perforation made by the nerve, and their difference was exactly one-fifth of an inch. To this we must add a fiftieth, on account of the eccentricity of the pupil in the uvea, which in the eye that I measured was not great, and the distance of the centre of the nerve from the point opposite the pupil will be 11 hundredths. Hence it appears that the visual axis is five hundredths, or one-twentieth of an inch, further from the optic nerve than the point opposite the pupil. It is possible that this distance may be different in different eyes; in mine, the obliquity of the lens, and the eccentricity of the pupil with respect to it, will tend to throw a direct ray upon it, without much inclination of the whole eye; and it is not improbable, that the eye is also turned slightly outwards, if looking at any object before it, although the inclination is too small to be subjected to measurement.

It must also be observed, that it is very difficult to ascertain the proportions of the eye so exactly as to determine, with certainty, the size of an image on the retina; the situation, curvature, and constitution of the lens, make so material a difference in the result, that there may possibly be an error of almost one-tenth of the whole. In order, therefore, to obtain some confirmation from experiment, I placed two candles at a small distance from each other, turned the eye inwards, and applied the ring of a key so as to produce a spectrum, of which the edge coincided with the inner candle; then, fixing my eye on the outward one, I found that the spectrum advanced over two-sevenths of the distance between them. Hence, the same portion of the retina that subtended an angle of seven parts at the centre of motion of the eye, subtended an angle of five at the supposed

intersection of the principal rays (Fig. 14); and the distance of this intersection from the retina was 637 thousandths. This nearly corresponds with the former calculation; nor can the distance of the centre of the optic nerve from the point of most perfect vision be, on any supposition, much less than that which is here assigned. And, in the eyes of quadrupeds, the most strongly painted part of the choroid is further from the nerve than the real axis of the eye.

I have endeavoured to express, in four figures, the form of every part of my eye, as nearly as I have been able to ascertain it; the first (Fig. 20) is a vertical section; the second (Fig. 21) a horizontal section; the third and fourth are front views, in different states of the pupil. (Fig. 22 and 23.)

Considering how little inconvenience is experienced from so material an inequality in the refraction of the lens as I have described, we have no reason to expect a very accurate provision for correcting the aberration of the lateral rays. But, as far as can be ascertained by the optometer, the aberration arising from figure is completely corrected; since four or more images of the same line appear to meet exactly in the same point, which they would not do if the lateral rays were materially more refracted than the rays near the axis. The figure of the surfaces is sometimes, and perhaps always, more or less hyperbolical * or elliptical: in the interior laminæ, indeed, the solid angle of the margin is somewhat rounded off; but the weaker refractive power of the external parts must greatly tend to correct the aberration arising from the too great curvature towards the margin of the disc. Had the refractive power been uniform, it might have collected the lateral rays of a direct pencil nearly as well; but it would have been less adapted to oblique pencils of rays; and the eye must also have been encumbered with a mass of much greater density than is now required, even for the central parts: and, if the whole lens had been smaller, it would also have admitted too little light. It is possible too, that Mr. Ramsden's observation,† on the advantage of having no reflecting surface, may be well founded: but it has not been demonstrated, that less

* Petit, Mém. de l'Acad. 1725, p. 20. † Phil. Trans. for 1795, p. 2.

light is lost in passing through a medium of variable density, than in a sudden transition from one part of that medium to another; nor are we yet sufficiently acquainted with the cause of this reflection, to be enabled to reason satisfactorily on the subject. But neither this gradation, nor any other provision, has the effect of rendering the eye perfectly achromatic. Dr. Jurin had remarked this long ago,* from observing the colour bordering the image of an object seen indistinctly. Dr. Wollaston pointed out to me on the optometer, the red and blue appearance of the opposite internal angles of the crossing lines; and mentioned, at the same time, a very elegant experiment for proving the dispersive power of the eye. He looks through a prism at a small lucid point, which of course becomes a linear spectrum. But the eye cannot so adapt itself as to make the whole spectrum appear a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted, and expand into a surface; and the reverse will happen if the eye be adapted to the blue rays; so that, in either case, the line will be seen as a triangular space. The observation is confirmed by placing a small concave speculum in different parts of a prismatic spectrum, and ascertaining the utmost distances at which the eye can collect the rays of different colours to a focus. By these means I find, that the red rays, from a point at 12 inches distance, are as much refracted as white or yellow light at 11. The difference is equal to the refraction of a lens 132 inches in focus. But the aberration of the red rays in a lens of crown glass, of equal mean refractive power with the eye, would be equivalent to the effect of a lens 44 inches in focus. If, therefore, we can depend upon this calculation, the dispersive power of the eye collectively is one-third of the dispersive power of crown glass, at an equal angle of deviation. I cannot observe much aberration in the violet rays. This may be, in part, owing to their faintness; but yet I think their aberration must be less than that of the red rays. I believe it was Mr. Ramsden's opinion, that since the separation of coloured rays is only observed where there is a sudden change of density, such a body as the

* Smith, c. 96.

lens, of a density gradually varying, would have no effect whatever in separating the rays of different colours. If this hypothesis should appear to be well-founded, we must attribute the whole dispersion to the aqueous humour; and its dispersive power will be half that of crown glass, at the same deviation. But we have an instance in the atmosphere of a very gradual change of density; and yet Mr. Gilpin informs me that the stars, when near the horizon, appear very evidently coloured. At a more favourable season of the year, it would not be difficult to ascertain, by means of the optometer, the dispersive power of the eye, and of its different parts, with greater accuracy than by the experiment here related. Had the dispersive power of the whole eye been equal to that of flint glass, the distance of perfect vision would have varied from 12 inches to 7 for different rays, in the same state of the mean refractive powers.

VII.—The faculty of accommodating the eye to various distances, appears to exist in very different degrees in different individuals. The shortest distance of perfect vision in my eye is 26 tenths of an inch for horizontal, and 29 for vertical rays. This power is equivalent to the addition of a lens of 4 inches focus. Dr. Wollaston can see at 7 inches, and with converging rays; the difference answering to 6 inches focal length. Mr. Abernethy has perfect vision from 3 inches to 30, or a power equal to that of a lens $3\frac{1}{3}$ inches in focus. A young lady of my acquaintance can see at 2 inches and at 4; the difference being equivalent to 4 inches focus. A middle-aged lady at 3 and at 4; the power of accommodation being only equal to the effect of a lens of 12 inches focus. In general I have reason to think that the faculty diminishes in some degree as persons advance in life; but some also of a middle age appear to possess it in a very small degree. I shall take the range of my own eye, as being probably about the medium, and inquire what changes will be necessary in order to produce it; whether we suppose the radius of the cornea to be diminished, or the distance of the lens from the retina to be increased, or these two causes to act conjointly, or the figure of the lens itself to undergo an alteration.

1. We have calculated that when the eye is in a state of relaxation, the refraction of the cornea is such as to collect rays diverging from a point ten inches distant, to a focus at the distance of 13$\frac{8}{9}$ tenths. In order that it may bring to the same focus rays diverging from a point distant 29 tenths, we find (by Cor. 5. Prop. IV.) that its radius must be diminished from 31 to 25 hundredths, or very nearly in the ratio of five to four.

2. Supposing the change from perfect vision at 10 inches to 29 tenths, to be effected by a removal of the retina to a greater distance from the lens, this will require (by the same Corollary) an elongation of 135 thousandths, or more than one-seventh of the diameter of the eye. In Mr. Abernethy's eye an elongation of 17 hundredths, or more than one-sixth, is requisite.

3. If the radius of the cornea be diminished one-sixteenth, or to 29 hundredths, the eye must at the same time be elongated 97 thousandths, or about one-ninth of its diameter.

4. Supposing the crystalline lens to change its form; if it became a sphere, its diameter would be 28 hundredths, and, its anterior surface retaining its situation, the eye would have perfect vision at the distance of an inch and a half. (Cor. 5 and 8. Prop. IV.) This is more than double the actual change. But it is impossible to determine precisely how great an alteration of form is necessary, without ascertaining the nature of the curves into which its surfaces may be changed. If it were always a spheroid more or less oblate, the focal length of each surface would vary inversely as the square of the axis: but, if the surfaces became, from spherical, portions of hyperbolic conoids, or of oblong spheroids, or changed from more obtuse to more acute figures of this kind, the focal length would vary more rapidly. Disregarding the elongation of the axis, and supposing the curvature of each surface to be changed proportionally, the radius of the anterior must become about 21, and that of the posterior 15 hundredths.

VIII.—I shall now proceed to inquire, which of these changes takes place in nature; and I shall begin with a relation of experiments made in order to ascertain the curvature of the cornea in all circumstances.

The method described in Mr. Home's Croonian Lecture for 1795,* appears to be far preferable to the apparatus of the preceding year:† for a difference in the distance of two images seen in the cornea, would be far greater and more conspicuous, than a change of its prominency, and far less liable to be disturbed by accidental causes. It is nearly, and perhaps totally impossible to change the focus of the eye, without some motion of its axis. The eyes sympathize perfectly with each other; and the change of focus is almost inseparable from a change of the relative situation of the optic axes; so much, that if I direct both my eyes at an object beyond their furthest focus, I cannot avoid bringing that focus a little nearer: while one axis moves, it is not easy to keep the other perfectly at rest; and it is not impossible, that a change in the proportions of some eyes may render a slight alteration of the position of the axis absolutely necessary. These considerations may partly explain the trifling difference in the place of the cornea that was observed in 1794. It appears that the experiments of 1795 were made with considerable accuracy, and no doubt with excellent instruments; and their failing to ascertain the existence of any change, induced Mr. Home and Mr. Ramsden to abandon, in great measure, the opinion which suggested them, and to suppose, that a change of the cornea produces only one-third of the effect. Dr. Olbers of Bremen, who in the year 1780 published a most elaborate dissertation on the internal changes of the eye,‡ which he lately presented to the Royal Society, had been equally unsuccessful in his attempts to measure this change of the cornea, at the same time that his opinion was in favour of its existence.

Room was however still left for a repetition of the experiments; and I began with an apparatus nearly resembling that which Mr. Home has described. I had an excellent achromatic microscope, made by Mr. Ramsden for my friend Mr. John Ellis, of five inches focal length, magnifying about 20 times. To this I adapted a cancellated micrometer, in the focus of the eye not employed in looking through the microscope: it was

* Phil. Trans. for 1796, p. 2. † Phil. Trans. for 1795, p. 13.
‡ De Oculi Mutationibus internis. Götting. 1780. 4°.

a large card divided by horizontal and vertical lines into fortieths of an inch. When the image in the microscope was compared with this scale, care was taken to place the head so that the relative motion of the images on the micrometer, caused by the unsteadiness of the optic axis, should always be in the direction of the horizontal lines, and that there could be no error, from this motion, in the dimensions of the image taken vertically. I placed two candles so as to exhibit images in a vertical position in the eye of Mr. König, who had the goodness to assist me; and, having brought them into the field of the microscope, where they occupied 35 of the small divisions, I desired him to fix his eye on objects at different distances in the same direction: but I could not perceive the least variation in the distance of the images.

Finding a considerable difficulty in a proper adjustment of the microscope, and being able to depend on my naked eye in measuring distances, without an error of one 500th of an inch, I determined to make a similar experiment without any magnifying power. I constructed a divided eye-glass of two portions of a lens, so small, that they passed between two images reflected from my own eye; and, looking in a glass, I brought the apparent places of the images to coincide, and then made the change requisite for viewing nearer objects: but the images still coincided. Neither could I observe any change in the images reflected from the other eye, where they could be viewed with greater convenience, as they did not interfere with the eye-glass. But, not being at that time aware of the perfect sympathy of the eyes, I thought it most certain to confine my observation to the one with which I saw. I must remark, that by a little habit, I have acquired a very ready command over the accommodation of my eye, so as to be able to view an object with attention, without adjusting my eye to its distance.

I also stretched two threads, a little inclined to each other, across a ring, and divided them by spots of ink into equal spaces. I then fixed the ring, applied my eye close behind it, and placed two candles in proper situations before me, and a third on one side, to illuminate the threads. Then setting a

small looking-glass, first at four inches distance and next at two, I looked at the images reflected in it, and observed at what part of the threads they exactly reached across in each case; and with the same result as before.

I next fixed the cancellated micrometer at a proper distance, illuminated it strongly, and viewed it through a pin-hole, by which means it became distinct in every state of the eye; and, looking with the other eye into a small glass, I compared the image with the micrometer, in the manner already described. I then changed the focal distance of the eye, so that the lucid points appeared to spread into surfaces, from being too remote for perfect vision; and I noted on the scale the distance of their centres; but that distance was invariable.

Lastly, I drew a diagonal scale, with a diamond, on a looking-glass (Fig. 15), and brought the images into contact with the lines of the scale. Then, since the image of the eye occupies on the surface of a glass half its real dimensions, at whatever distance it is viewed, its true size is always double the measure thus obtained. I illuminated the glass strongly, and made a perforation in a narrow slip of black card, which I held between the images; and was thus enabled to compare them with the scale, although their apparent distance was double that of the scale. I viewed them in all states of the eye; but I could perceive no variation in the interval between them.

The sufficiency of these methods may be thus demonstrated. Make a pressure along the edge of the upper eyelid with any small cylinder, for instance a pencil, and the optometer will show that the focus of horizontal rays is a little elongated, while that of vertical rays is shortened; an effect which can only be owing to a change of curvature in the cornea. Not only the apparatus here described, but even the eye unassisted, will be capable of discovering a considerable change in the images reflected from the cornea, although the change be much smaller than that which is requisite for the accommodation of the eye to different distances. On the whole, I cannot hesitate to conclude, that if the radius of the cornea were diminished but one-twentieth, the change would be very readily percep-

tible by some of the experiments related; and the whole alteration of the eye requires one-fifth.

But a much more accurate and decisive experiment remains. I take out of a small botanical microscope, a double convex lens, of eight-tenths radius and focal distance, fixed in a socket one-fifth of an inch in depth; securing its edges with wax, I drop into it a little water, nearly cold, till it is three-fourths full, and then apply it to my eye, so that the cornea enters half way into the socket, and is everywhere in contact with the water. (Fig. 16.) My eye immediately becomes presbyopic, and the refractive power of the lens, which is reduced by the water to a focal length of about 16 tenths (Cor. 5. Prop. IV.), is not sufficient to supply the place of the cornea, rendered inefficacious by the intervention of the water; but the addition of another lens, of five inches and a half focus, restores my eye to its natural state, and somewhat more. I then apply the optometer, and I find the same inequality in the horizontal and vertical refractions as without the water; and I have, in both directions, a power of accommodation equivalent to a focal length of four inches, as before. At first sight indeed, the accommodation appears to be somewhat less, and only able to bring the eye from the state fitted for parallel rays to a focus at five inches distance; and this made me once imagine, that the cornea might have some slight effect in the natural state; but, considering that the artificial cornea was about a tenth of an inch before the place of the natural cornea, I calculated the effect of this difference, and found it exactly sufficient to account for the diminution of the range of vision. I cannot ascertain the distance of the glass lens from the cornea to the hundredth of an inch; but the error cannot be much greater, and it may be on either side.

After this it is almost necessary to apologize for having stated the former experiments; but, in so delicate a subject, we cannot have too great a variety of concurring evidence.

IX.—Having satisfied myself that the cornea is not concerned in the accommodation of the eye, my next object was to inquire if any alteration in the length of its axis could be discovered;

for this appeared to be the only possible alternative: and, considering that such a change must amount to one-seventh of the diameter of the eye, I flattered myself with the expectation of submitting it to measurement. Now, if the axis of the eye were elongated one-seventh, its transverse diameter must be diminished one-fourteenth, and the semi-diameter would be shortened a thirtieth of an inch.

I therefore placed two candles so that when the eye was turned inwards, and directed towards its own image in a glass, the light reflected from one of the candles by the sclerotica appeared upon its external margin, so as to define it distinctly by a bright line; and the image of the other candle was seen in the centre of the cornea. I then applied the double eye-glass, and the scale of the looking-glass, in the manner already described; but neither of them indicated any diminution of the distance, when the focal length of the eye was changed.

Another test, and a much more delicate one, was the application of the ring of a key at the external angle, when the eye was turned as much inwards as possible, and confined at the same time by a strong oval iron ring, pressed against it at the internal angle. The key was forced in as far as the sensibility of the integuments would admit, and was wedged, by a moderate pressure, between the eye and the bone. In this situation the phantom caused by the pressure extended within the field of perfect vision, and was very accurately defined; nor did it, as I formerly imagined, by any means prevent a distinct perception of the objects actually seen in that direction; and a straight line coming within the field of this oval phantom, appeared somewhat inflected towards its centre (Fig. 17); a distortion easily understood by considering the effect of the pressure on the form of the retina. Supposing now, the distance between the key and the iron ring to have been, as it really was, invariable, the elongation of the eye must have been either totally or very nearly prevented; and, instead of an increase of the length of the eye's axis, the oval spot caused by the pressure would have spread over a space at least ten times as large as the most sensible part of the retina. But no such circumstance took place: the power of accommodation

was as extensive as ever; and there was no perceptible change either in the size or in the figure of the oval spot.

Again, since the rays which pass through the centre of the pupil, or rather the anterior vertex of the lens, may, as already observed, be considered as delineating the image; and, since the divergence of these rays with respect to each other, is but little affected by the refraction of the lens, they may still be said to diverge from the centre of the pupil; and the image of a given object on the retina must be very considerably enlarged, by the removal of the retina to a greater distance from the pupil and lens. (Cor. Prop. V.) To ascertain the real magnitude of the image with accuracy is not so easy as it at first sight appears; but, besides the experiment last related, which might be employed as an argument to this purpose, there are two other methods of estimating it. The first is too hazardous to be of much use; but, with proper precautions, it may be attempted. I fix my eye on a brass circle placed in the rays of the sun, and, after some time, remove it to the cancellated micrometer; then changing the focus of my eye, while the micrometer remains at a given distance, I endeavour to discover whether there is any difference in the apparent magnitude of the spectrum on the scale; but I can discern none. I have not insisted on the attempt; especially as I have not been able to make the spectrum distinct enough without inconvenience; and no light is sufficiently strong to cause a permanent impression on any part of the retina remote from the visual axis. I therefore had recourse to another experiment. I placed two candles so as exactly to answer to the extent of the termination of the optic nerve, and, marking accurately the point to which my eye was directed, I made the utmost change in its focal length; expecting that, if there were any elongation of the axis, the external candle would appear to recede outwards upon the visible space. (Fig. 18.) But this did not happen; the apparent place of the obscure part was precisely the same as before. I will not undertake to say, that I could have observed a very minute difference either way: but I am persuaded, that I should have discovered an alteration of less than a tenth part of the whole.

It may be inquired if no change in the magnitude of the image is to be expected on any other supposition; and it will appear to be possible, that the changes of curvature may be so adapted, that the magnitude of the confused image may remain perfectly constant. Indeed, to calculate from the dimensions which we have hitherto used, it would be expected that the image should be diminished about one-fortieth, by the utmost increase of the convexity of the lens. But the whole depends on the situation of the refracting surfaces, and the respective increase of their curvature, which, on account of the variable density of the lens, can scarcely be estimated with sufficient accuracy. Had the pupil been placed before the cornea, the magnitude of the image must, on any supposition, have been very variable: at present, this inconvenience is avoided by the situation of the pupil; so that we have here an additional instance of the perfection of this admirable organ.

From the experiments related, it appears to be highly improbable that any material change in the length of the axis actually takes place; and it is almost impossible to conceive by what power such a change could be effected. The straight muscles, with the adipose substance lying under them, would certainly, when acting independently of the socket, tend to flatten the eye: for, since their contraction would necessarily lessen the circumference or superficies of the mass that they contain, and round off all its prominences, their attachment about the nerve and the anterior part of the eye must therefore be brought nearer together. (Fig. 24, 25.) Dr. Olbers compares the muscles and the eye to a cone of which the sides are protruded, and would by contraction be brought into a straight line. But this would require a force to preserve the cornea as a fixed point, at a given distance from the origin of the muscles; a force which certainly does not exist. In the natural situation of the visual axis, the orbit being conical, the eye might be somewhat lengthened, although irregularly, by being forced further into it; but, when turned towards either side, the same action would rather shorten its axis; nor is there anything about the human eye that could supply its place. In quadrupeds the oblique muscles are wider than in man;

and in many situations might assist in the effect. Indeed a portion of the orbicular muscle of the globe is attached so near to the nerve, that it might also co-operate in the action: and I have no reason to doubt the accuracy of Dr. Olbers, who states, that he effected a considerable elongation, by tying threads to the muscles, in the eyes of hogs and of calves; yet he does not say in what position the axis was fixed; and the flaccidity of the eye after death might render such a change very easy as would be impossible in a living eye. Dr. Olbers also mentions an observation of Professor Wrisberg, on the eye of a man whom he believed to be destitute of the power of accommodation in his life-time, and whom he found, after death, to have wanted one or more of the muscles: but this want of accommodation was not at all accurately ascertained. I measured, in the human eye, the distance of the attachment of the inferior oblique muscle from the insertion of the nerve: it was one-fifth of an inch; and from the centre of vision not a tenth of an inch; so that, although the oblique muscles do in some positions nearly form a part of a great circle round the eye, their action would be more fitted to flatten than to elongate it. We have therefore reason to agree with Winslow, in attributing to them the office of helping to support the eye on that side where the bones are most deficient: they seem also well calculated to prevent its being drawn too much backwards by the action of the straight muscles. And, even if there were no difficulty in supposing the muscles to elongate the eye in every position, yet at least some small difference would be expected in the extent of the change, when the eye is in different situations, at an interval of more than a right angle from each other; but the optometer shows that there is none.

Dr. Hosack alleges that he was able, by making a pressure on the eye, to accommodate it to a nearer object:* it does not appear that he made use of very accurate means of ascertaining the fact; but, if such an effect took place, the cause must have been an inflection of the cornea.

It is unnecessary to dwell on the opinion which supposes a joint operation, of changes in the curvature of the cornea and

* Phil. Trans. for 1794, p. 212.

in the length of the axis. This opinion had derived very great respectability, from the most ingenious and elegant manner in which Dr. Olbers had treated it, and from being the last result of the investigation of Mr. Home and Mr. Ramsden. But either of the series of experiments which have been related, appears to be sufficient to confute it.

X.—It now remains to inquire into the pretensions of the crystalline lens to the power of altering the focal length of the eye. The grand objection to the efficacy of a change of figure in the lens was derived from the experiments in which those who have been deprived of it have appeared to possess the faculty of accommodation.

My friend Mr. Ware, convinced as he was of the neatness and accuracy of the experiments related in the Croonian Lecture for 1795, yet could not still help imagining, from the obvious advantage all his patients found, after the extraction of the lens, in using two kinds of spectacles, that there must, in such cases, be a deficiency in that faculty. This circumstance, combined with a consideration of the directions very judiciously given by Dr. Porterfield, for ascertaining the point in question, first made me wish to repeat the experiments upon various individuals, and with the instrument which I have above described as an improvement of Dr. Porterfield's optometer: and I must here acknowledge my great obligation to Mr. Ware, for the readiness and liberality with which he introduced me to such of his numerous patients as he thought most likely to furnish a satisfactory determination. It is unnecessary to enumerate every particular experiment; but the universal result is, contrary to the expectation with which I entered on the inquiry, that in an eye deprived of the crystalline lens, the actual focal distance is totally unchangeable. This will appear from a selection of the most decisive observations.

1. Mr. R. can read at four inches, and at six only, with the same glass. He saw the double lines meeting at three inches, and always at the same point; but the cornea was somewhat irregularly prominent, and his vision not very distinct; nor had I, at the time I saw him, a convenient apparatus.

I afterwards provided a small optometer, with a lens of less than two inches focus, adding a series of letters, not in alphabetical order, and projected into such a form as to be most legible at a small inclination. The excess of the magnifying power had the advantage of making the lines more divergent, and their crossing more conspicuous; and the letters served for more readily naming the distance of the intersection, and, at the same time, for judging of the extent of the power of distinguishing objects too near or too remote for perfect vision. (Fig. 26.)

2. Mr. J. had not an eye very proper for the experiment; but he appeared to distinguish the letters at 2½ inches, and at less than an inch. This at first persuaded me, that he must have a power of changing the focal distance: but I afterwards recollected that he had withdrawn his eye considerably, to look at the nearer letters, and had also partly closed his eyelids, no doubt contracting at the same time the aperture of the pupil; an action which, even in a perfect eye, always accompanies the change of focus. The slider was not applied.

3. Miss H., a young lady of about twenty, had a very narrow pupil, and I had not an opportunity of trying the small optometer: but, when she once saw an object double through the slits, no exertion could make it appear single at the same distance. She used for distant objects a glass of 4½ inches focus; with this she could read as far off as 12 inches, and as near as 5: for nearer objects she added another of equal focus, and could then read at 7 inches, and at 2½.

4. Hanson, a carpenter, aged 63, had a cataract extracted a few years since from one eye: the pupil was clear and large, and he saw well to work with a lens of 2⅜ inches focus; and could read at 8 and at 15 inches, but most conveniently at 11. With the same glass, the lines of the optometer appeared always to meet at 11 inches; but he could not perceive that they crossed, the line being too strong, and the intersection too distant. The experiment was afterwards repeated with the small optometer: he read the letters from 2 to 3 inches; but the intersection was always at 2½ inches. He now fully understood the circumstances that were to be noticed, and saw the

crossing with perfect distinctness: at one time, he said it was a tenth of an inch nearer; but I observed that he had removed his eye two or three tenths from the glass, a circumstance which accounted for this small difference.

5. Notwithstanding Hanson's age, I consider him as a very fair subject for the experiment. But a still more unexceptionable eye was that of Mrs. Maberly. She is about 30, and had the crystalline of both eyes extracted a few years since, but sees best with her right. She walks without glasses; and, with the assistance of a lens, of about four inches focus, can read and work with ease. She could distinguish the letters of the small optometer from an inch to 2½ inches; but the intersection was invariably at the same point, about 19 tenths of an inch distant. A portion of the capsule is stretched across the pupil, and causes her to see remote objects double, when without her glasses; nor can she, by any exertion, bring the two images nearer together, although the exertion makes them more distinct, no doubt by contracting the pupil. The experiment with the optometer was conducted, in the presence of Mr. Ware, with patience and perseverance; nor was any opinion given to make her report partial.

Considering the difficulty of finding an eye perfectly suitable for the experiments, these proofs may be deemed tolerably satisfactory. But, since one positive argument will counterbalance many negative ones, provided it be equally grounded on fact, it becomes necessary to inquire into the competency of the evidence employed to ascertain the power of accommodation attributed, in the Croonian Lecture for 1794, to the eye of Benjamin Clerk. And it appears, that the distinction long since very properly made by Dr. Jurin, between distinct vision and perfect vision, will readily explain away the whole of that evidence.

It is obvious that vision may be made distinct to any given extent, by means of an aperture sufficiently small, provided, at the same time, that a sufficient quantity of light be left, while the refractive powers of the eye remain unchanged. And it is remarkable, that in those experiments, when the comparison with the perfect eye was made, the aperture of the imperfect

eye only was very considerably reduced. Benjamin Clerk, with an aperture of $\frac{8}{40}$ of an inch, could read with the same glass at $1\frac{7}{8}$ inch, and at 7 inches.* With an equal aperture, I can read at $1\frac{1}{2}$ inch and at 30 inches; and I can retain the state of perfect relaxation, and read with the same aperture at $2\frac{1}{4}$ inches; and this is as great a difference as was observed in Benjamin Clerk's eye. It is also a fact of no small importance, that Sir Henry Englefield was much astonished, as well as the other observers, at the accuracy with which the man's eye was adjusted to the same distance, in the repeated trials that were made with it.† This circumstance alone makes it highly probable, that its perfect vision was confined within very narrow limits.

Hitherto I have endeavoured to show the inconveniences attending other suppositions, and to remove the objections to the opinion of an internal change of the figure of the lens. I shall now state two experiments, which, in the first place, come very near to a mathematical demonstration of the existence of such a change, and, in the second, explain in great measure its origin, and the manner in which it is effected.

I have already described the appearances of the imperfect image of a minute point at different distances from the eye, in a state of relaxation. For the present purpose, I will only repeat, that if the point is beyond the furthest focal distance of the eye, it assumes that appearance which is generally described by the name of a star, the central part being considerably the brightest. (Fig. 39—42.) But, when the focal distance of the eye is shortened, the imperfect image is of course enlarged; and, besides this necessary consequence, the light is also very differently distributed; the central part becomes faint, and the margin strongly illuminated, so as to have almost the appearance of an oval ring. (Fig. 44.) If I apply the slider of the optometer, the shadows of the slits, while the eye is relaxed, are perfectly straight, dividing the oval either way into parallel segments (Fig. 45, 47): but when the accommodation takes place, they immediately become curved, and the more so the further they are from the centre of the image, to which

* Phil. Trans. for 1795, p. 9. † Phil. Trans. for 1795, p. 8.

their concavity is directed. (Fig. 46, 48.) If the point be brought much within the focal distance, the change of the eye will increase the illumination of the centre, at the expense of the margin. The same appearances are equally observable, when the effect of the cornea is removed by immersion in water; and the only imaginable way of accounting for the diversity, is to suppose the central parts of the lens to acquire a greater degree of curvature than the marginal parts. If the refraction of the lens remained the same, it is absolutely impossible that any change of the distance of the retina should produce a curvature in those shadows, which, in the relaxed state of the eye, are found to be in all parts straight; and that neither the form nor the relative situation of the cornea is concerned, appears from the application of water already mentioned.

The truth of this explanation is fully confirmed by the optometer. When I look through four narrow slits, without exertion, the lines always appear to meet in one point: but when I make the intersection approach me, the two outer lines meet considerably beyond the inner ones, and the two lines of the same side cross each other at a still greater distance. (Fig. 27.)

The experiment will not succeed with every eye: nor can it be expected that such an imperfection should be universal: but one case is sufficient to establish the argument, even if no other were found. I do not however doubt, that in those who have a large pupil, the aberration may be very frequently observable. In Dr. Wollaston's eye, the diversity of appearance is imperceptible; but Mr. König described the intersections exactly as they appear to me, although he had received no hint of what I had observed. The lateral refraction is the most easily ascertained, by substituting for the slits a tapering piece of card, so as to cover all the central parts of the pupil, and thus determining the nearest crossing of the shadows transmitted through the marginal parts only. When the furthest intersection was at 38, I could bring it to 22 parts with two narrow slits; but with the tapered card only to 2[illegible]. From these data we may determine pretty nearly into what form the lens must be changed, supposing both the surfaces to undergo proportional alterations of curvature, and taking for granted the dimensions already

laid down: for, from the lateral aberration thus given, we may find (by Prop. III.) the subtangents at about one-tenth of an inch from the axis; and the radius of curvature at each vertex is already determined to be about 21 and 15 hundredths of an inch. Hence the anterior surface must be a portion of a hyperboloid, of which the greater axis is about 50; and the posterior surface will be nearly parabolical. In this manner the change will be effected, without any diminution of the transverse diameter of the lens. The elongation of its axis will not exceed the fiftieth of an inch; and on the supposition with which we set out, the protrusion will be chiefly at the posterior vertex. The form of the lens thus changed will be nearly that of Fig. 29; the relaxed state being nearly as represented in Fig. 28. Should, however, the rigidity of the internal parts, or any other considerations, render it convenient to suppose the anterior surface more changed, it would still have room, without interfering with the uvea; or it might even force the uvea a little forwards, without any visible alteration of the external appearance of the eye.

From this investigation of the change of the figure of the lens, it appears that the action which I formerly attributed to the external coats, cannot afford an explanation of the phenomenon. The necessary effect of such an action would be, to produce a figure approaching to that of an oblate spheroid; and, to say nothing of the inconvenience attending a diminution of the diameter of the lens, the lateral refraction would be much more increased than the central; nor would the slight change of density, at an equal distance from the axis, be at all equivalent to the increase of curvature: we must therefore suppose some different mode of action in the power producing the change. Now, whether we call the lens a muscle or not, it seems demonstrable, that such a change of figure takes place as can be produced by no external cause; and we may at least illustrate it by a comparison with the usual action of muscular fibres. A muscle never contracts, without at the same time swelling laterally, and it is of no consequence which of the effects we consider as primary. I was induced, by an occasional opacity, to give the name of

membranous tendons to the radiations from the centre of the lens; but, on a more accurate examination, nothing really analogous to tendon can be discovered. And, if it were supposed that the parts next the axis were throughout of a tendinous, and therefore unchangeable nature, the contraction must be principally effected by the lateral parts of the fibres; so that the coats would become thicker towards the margin, by their contraction, while the general alteration of form would require them to be thinner; and there would be a contrariety in the actions of the various parts. But, if we compare the central parts of each surface to the belly of the muscle, there is no difficulty in conceiving their thickness to be immediately increased, and to produce an immediate elongation of the axis, and an increase of the central curvature; while the lateral parts co-operate more or less, according to their distance from the centre, and in different individuals in somewhat different proportions. On this supposition, we have no longer any difficulty in attributing a power of change to the crystalline of fishes. M. Petit, in a great number of observations, uniformly found the lens of fishes more or less flattened: but even if it were not, a slight extension of the lateral part of the superficial fibres would allow those softer coats to become thicker at each vertex, and to form the whole lens into a spheroid somewhat oblong; and here, the lens being the only agent in refraction, a less alteration than in other animals would be sufficient. It is also worthy of inquiry, whether the state of contraction may not immediately add to the refractive power. According to the old experiment, by which Dr. Goddard attempted to show that muscles become more dense as they contract, such an effect might naturally be expected. That experiment is, however, very indecisive, and the opinion is, indeed, generally exploded, but perhaps too hastily; and whoever shall ascertain the existence or non-existence of such a condensation, will render essential service to physiology in general.

Dr. Pemberton, in the year 1719, first systematically discussed the opinion of the muscularity of the crystalline lens.*

* De Facultate Oculi qua ad diversas Rerum distantias se accommodat. L.B. 1719. Ap. Hall. Disp. Anat. iv. p. 301.

He referred to Lecuwenhoek's microscopical observations; but he so overwhelmed his subject with intricate calculations, that few have attempted to develop it: and he grounded the whole on an experiment borrowed from Barrow, which with me has totally failed; and I cannot but agree with Dr. Olbers in the remark, that it is easier to confute him than to understand him. He argued for a partial change o) the figure of the lens; and perhaps the opinion was more just than the reasons adduced for its support. Lobé, or rather Albinus,* decidedly favours a similar theory; and suggests the analogy of the lens to the muscular parts of pellucid animals, in which even the best microscopes can discover no fibres. Camper also mentions the hypothesis with considerable approbation.† Professor Reil published, in 1793, a Dissertation on the Structure of the Lens; and, in a subsequent paper, annexed to the translation of my former Essay in Professor Gren's Journal,‡ he discussed the question of its muscularity. I regret that I have not now an opportunity of referring to this publication; but I do not recollect that Professor Reil's objections are different from those which I have already noticed.

Considering the sympathy of the crystalline lens with the uvea, and the delicate nature of the change of its figure, there is little reason to expect that any artificial stimulus would be more successful in exciting a contractive action in the lens, than it has hitherto been in the uvea; much less would that contraction be visible without art. Soon after Mr. Hunter's death, I pursued the experiment, which he had suggested, for ascertaining how far such a contraction might be observable. My apparatus (Fig. 30) was executed by Mr. Jones. It consisted of a wooden vessel blacked within, which was to be filled with cool, and then with warmer water: a plane speculum was placed under it; a perforation in the bottom was filled with a plate of glass; proper rings were fixed for the reception of the lens, or of the whole eye, and also wires for transmitting electricity: above these, a piece of ground and painted glass,

* De quibusdam Oculi Partibus. L. B. 1746. Ap. Hall. Disp. Anat. iv. p. 301.
† De Oculo Humano. L. B. 1742. Ap. Hall. Disp. Anat. vii. 2. p. 108, 109.
‡ 1794, p. 352, 354.

for receiving the image, was supported by a bracket, which moved by a pivot, in connection with a scale divided into fiftieths of an inch. With this apparatus I made some experiments, assisted by Mr. Wilkinson, whose residence was near a slaughter-house: but we could obtain by this method no satisfactory evidence of the change; nor was our expectation much disappointed. I understand also, that another member of this Society was equally unsuccessful, in attempting to produce a conspicuous change in the lens by electricity.

XI.—In man and in the most common quadrupeds, the structure of the lens is nearly similar. The number of radiations is of little consequence; but I find that in the human crystalline there are ten on each side (Fig. 49), not three, as I once, from a hasty observation, concluded.* Those who find any difficulty in discovering the fibres, must have a sight very ill adapted to microscopical researches. I have laboured with the most obstinate perseverance to trace nerves into the lens, and I have sometimes imagined that I had succeeded; but I cannot positively go further than to state my full conviction of their existence, and of the precipitancy of those who have absolutely denied it. The long nerves, which are very conspicuous between the choroid and sclerotic coats, divide each into two, three, or more branches, at the spot where the ciliary zone begins, and seem indeed to furnish the choroid with some fine filaments at the same place. The branches often re-unite, with a slight protuberance, that scarcely deserves the name of a ganglion; here they are tied down, and mixed with the hard whitish-brown membrane that covers the compact spongy substance in which the vessels of the ciliary processes anastomose and subdivide. (Fig. 50.) The quantity of the nerves which proceeds to the iris appears to be considerably smaller than that which arrives at the place of division: hence there can be little doubt that the division is calculated to supply the lens with some minute branches; and it is not improbable, from the appearance of the parts, that some fibres may pass to the cornea; although it might more naturally be expected, that

* De Corp. Hum. Vir. Cons., p. 68.

the tunica conjunctiva would be supplied from without. But the subdivisions which probably pass to the lens, enter immediately into a mixture of ligamentous substance and of a tough brownish membrane; and I have not hitherto been able to develop them. Perhaps animals may be found in which this substance is of a different nature; and I do not despair that, with the assistance of injections, for more readily distinguishing the blood-vessels, it may still be possible to trace them in quadrupeds. Our inability to discover them is scarcely an argument against their existence: they must naturally be delicate and transparent; and we have an instance, in the cornea, of considerable sensibility, where no nerve has yet been traced. The capsule adheres to the ciliary substance, and the lens to the capsule, principally in two or three points; but I confess, I have not been able to observe that these points are exactly opposite to the trunks of nerves; so that, probably, the adhesion is chiefly caused by those vessels which are sometimes seen passing to the capsule in injected eyes. We may, however, discover ramifications from some of these points, upon and within the substance of the lens (Fig. 51), generally following a direction near to that of the fibres, and sometimes proceeding from a point opposite to one of the radiating lines of the same surface. But the principal vessels of the lens appear to be derived from the central artery, by two or three branches at some little distance from the posterior vertex; which I conceive to be the cause of the frequent adhesion of a portion of a cataract to the capsule, about this point: they follow nearly the course of the radiations, and then of the fibres; but there is often a superficial subdivision of one of the radii at the spot where one of them enters. The vessels coming from the choroid appear principally to supply a substance, hitherto unobserved, which fills up the marginal part of the capsule of the crystalline, in the form of a thin zone, and makes a slight elevation, visible even through the capsule (Fig. 52—54). It consists of coarser fibres than the lens, but in a direction nearly similar; they are often intermixed with small globules. In some animals, the margin of the zone is crenated, especially behind, where it is shorter: this is observ-

able in the partridge; and, in the same bird, the whole surface of the lens is seen to be covered with points, or rather globules, arranged in regular lines (Fig. 55), so as to have somewhat the appearance of a honeycomb, but towards the vertex less uniformly disposed. This regularity is a sufficient proof that there could be no optical deception in the appearance; although it requires a good microscope to discover it distinctly: but the zone may be easily peeled off under water, and hardened in spirits. Its use is uncertain; but it may possibly secrete the liquid of the crystalline; and it as much deserves the name of a gland, as the greater part of the substances usually so denominated. In peeling it off, I have very distinctly observed ramifications, which were passing through it into the lens (Fig. 53); and indeed it is not at all difficult to detect the vessels connecting the margin of the lens with its capsule; and it is surprising that M. Petit should have doubted of their existence. I have not yet clearly discerned this crystalline gland in the human eye; but I infer the existence of something similar to the globules, from the spotted appearance of the image of a lucid point already mentioned; for which I can no otherwise account, than by attributing it to a derangement of these particles, produced by the external force, and to an unequal impression made by them on the surface of the lens.

In birds and in fishes, the fibres of the crystalline radiate equally, becoming finer as they approach the vertex, till they are lost in a uniform substance, of the same degree of firmness, which appears to be perforated in the centre by a blood-vessel. (Fig. 56.) In quadrupeds, the fibres at their angular meeting are certainly not continued, as Leeuwenhoek imagined, across the line of division; but there does not appear to be any dissimilar substance interposed between them, except that very minute trunks of vessels often mark that line. But since the whole mass of the lens, as far as it is moveable, is probably endued with a power of changing its figure, there is no need of any strength of union, or place of attachment, for the fibres, since the motion meets with little or no resistance. Every common muscle, as soon as its contraction ceases, returns to

its natural form, even without the assistance of an antagonist; and the lens itself, when taken out of the eye, in its capsule, has elasticity enough to reassume its proper figure, on the removal of a force that has compressed it. The capsule is highly elastic; and, since it is laterally fixed to the ciliary zone, it must co-operate in restoring the lens to its flattest form. If it be inquired why the lens is not capable of becoming less convex, as well as more so, it may be answered, that the lateral parts have probably little contractive power; and, if they had more, they would have no room to increase the size of the disc, which they must do, in order to shorten the axis; and the parts about the axis have no fibres so arranged as to shorten it by their own contraction.

I consider myself as being partly repaid for the labour lost in search of the nerves of the lens, by having acquired a more accurate conception of the nature and situation of the ciliary substance. It had already been observed, that in the hare and in the wolf, the ciliary processes are not attached to the capsule of the lens; and if by the ciliary processes we understand those filaments which are seen detached after tearing away the capsule, and consist of ramifying vessels, the observation is equally true of the common quadrupeds, and, I will venture to say, of the human eye.* Perhaps this remark has been made by others, but the circumstance is not generally understood. It is so difficult to obtain a distinct view of these bodies, undisturbed, that I am partly indebted to accident for having been undeceived respecting them: but, having once made the observation, I have learnt to show it in an unquestionable manner. I remove the posterior hemisphere of the sclerotica, or somewhat more, and also as much as possible of the vitreous humour, introduce the point of a pair of scissors into the capsule, turn out the lens, and cut off the greater part of the posterior portion of the capsule, and of the rest of the vitreous humour. I next dissect the choroid and uvea from the sclerotica; and, dividing the anterior part of the capsule into segments from its centre, I turn them back upon the ciliary zone. The ciliary processes then appear, covered with

* Vid. Hall. Physiol. v. p. 432, et Duverney, ibi citat.

their pigment, and perfectly distinct both from the capsule and from the uvea (Fig. 57); and the surface of the capsule is seen shining, and evidently natural, close to the base of these substances. I do not deny that the separation between the uvea and the processes extends somewhat further back than the separation between the processes and the capsule; but the difference is inconsiderable, and, in the calf, does not amount to above half the length of the detached part. The appearance of the processes is wholly irreconcileable with muscularity; and their being considered as muscles attached to the capsule is therefore doubly inadmissible. Their lateral union with the capsule commences at the base of their posterior smooth surface, and is continued nearly to the point where they are more intimately united with the termination of the uvea; so that, however this portion of the base of the processes were disposed to contract, it would be much too short to produce any sensible effect. What their use may be, cannot easily be determined; if it were necessary to have any peculiar organs for secretion, we might call them glands, for the percolation of the aqueous humour; but there is no reason to think them requisite for this purpose.

The marsupium nigrum of birds, and the horseshoe-like appearance of the choroid of fishes, are two substances which have sometimes, with equal injustice, been termed muscular. All the apparent fibres of the marsupium nigrum are, as Haller had very truly asserted, merely duplicatures of a membrane, which, when its ends are cut off, may easily be unfolded under the microscope, with the assistance of a fine hair pencil, so as to leave no longer any suspicion of a muscular texture. The experiment related by Mr. Home* can scarcely be deemed a very strong argument for attributing to this substance a faculty which its appearance so little authorises us to expect in it. The red substance in the choroid of fishes (Fig. 58) is more capable of deceiving the observer; its colour gives it some little pretension, and I began to examine it with a prepossession in favour of its muscular nature. But, when we recollect the general colour of the muscles of fishes,

* Phil. Trans. for 1796, p. 18.

the consideration of its redness will no longer have any weight. Stripped of the membrane which loosely covers its internal surface (Fig. 59), it seems to have transverse divisions, somewhat resembling those of muscles, and to terminate in a manner somewhat similar (Fig. 60); but, when viewed in a microscope, the transverse divisions appear to be cracks, and the whole mass is evidently of a uniform texture, without the least fibrous appearance; and, if a particle of any kind of muscle is compared with it, the contrast becomes very striking. Besides, it is fixed down, throughout its extent, to the posterior lamina of the choroid, and has no attachment capable of directing its effect; to say nothing of the difficulty of conceiving what that effect could be. Its use must remain, in common with that of many other parts of the animal frame, entirely concealed from our curiosity.

The bony scales of the eyes of birds, which were long ago described in the Philosophical Transactions by Mr. Ranby,* and by Mr. Warren,† afterwards in two excellent Memoirs of M. Petit on the eye of the turkey and of the owl,‡ and lately by Mr Pierce Smith,§ and Mr. Home,‖ can, on any supposition, have but little concern in the accommodation of the eye to different distances: they rather seem to be necessary for the protection of that organ, large and prominent as it is, and unsupported by any strength in the orbit, against the various accidents to which the mode of life and rapid motion of those animals must expose it; and they are much less liable to fracture than an entire bony ring of the same thickness would have been. The marsupium nigrum appears to be intended to assist in giving strength to the eye, to prevent any change in the place of the lens by external force; it is so situated as to intercept but little light, and that little is principally what would have fallen on the insertion of the optic nerve; and it seems to be too firmly tied to the lens, even to admit any considerable elongation of the axis of the eye, although it certainly would not impede a protrusion of the cornea.

* Phil. Trans. vol. xxxiii. p. 223, Abr. vol. vii. p. 435.
† Phil. Trans. vol. xxxiv. p. 113, Abr. vol. vii. p. 437.
‡ Mém. de l'Acad. 1735, p. 163; 1736, p. 166, Ed. Amst.
§ Phil. Trans. for 1795, p. 263. ‖ Phil. Trans. for 1796, p. 14.

With respect to the eyes of insects, an observation of Poupart deserves to be repeated here. He remarks that the eye of the libellula is hollow; that it communicates with an air-vessel placed longitudinally in the trunk of the body; and that it is capable of being inflated from this cavity: he supposes that the insect is provided with this apparatus, in order for the accommodation of its eye to the perception of objects at different distances.* I have not yet had an opportunity of examining the eye of the libellula; but there is no difficulty in supposing that the means of producing the change of the refractive powers of the eye may be, in different classes of animals, as diversified as their habits and the general conformation of their organs.

I beg leave to correct here an observation in my former paper, relative to the faint lateral radiations, which I supposed to proceed from the margin of the iris.† I find, on further examination, that they are occasioned by reflections from the eyelashes.

XII.—I shall now finally recapitulate the principal objects and results of the investigation which I have taken the liberty of detailing so fully to the Royal Society. First, the determination of the refractive power of a variable medium, and its application to the constitution of the crystalline lens. Secondly, the construction of an instrument for ascertaining, upon inspection, the exact focal distance of every eye, and the remedy for its imperfections. Thirdly, to show the accurate adjustment of every part of the eye, for seeing with distinctness the greatest possible extent of objects at the same instant. Fourthly, to measure the collective dispersion of coloured rays in the eye. Fifthly, by immerging the eye in water, to demonstrate that its accommodation does not depend on any change in the curvature of the cornea. Sixthly, by confining the eye at the extremities of its axis, to prove that no material alteration of its length can take place. Seventhly, to examine what inference can be drawn from the experiments hitherto made on persons deprived of the lens; to pursue the inquiry on the

* Phil. Trans. vol. xxii. p. 673, Abr. ii. p. 762.
† Supra, p. 9.

Fig. 4.

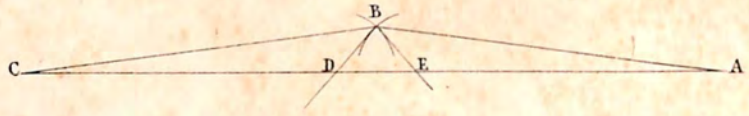

Fig. 5.

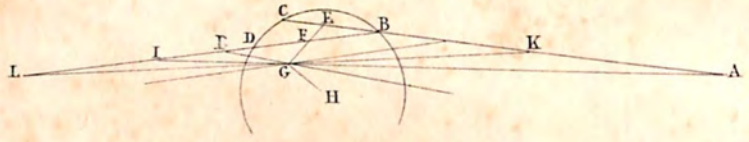

Fig. 6

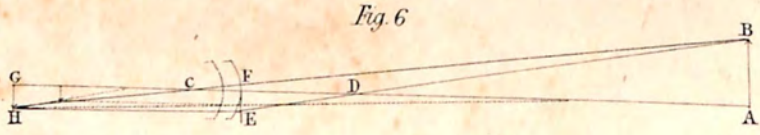

Fig. 7.

Fig. 8.

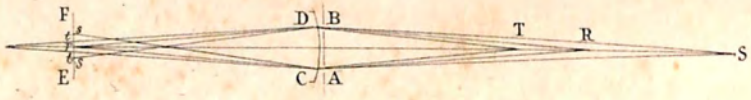

Fig. 9.

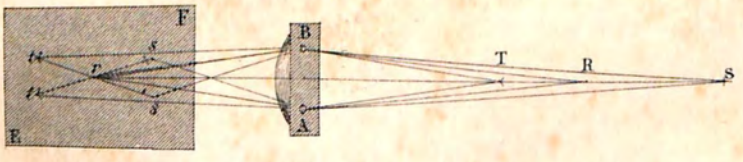

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Fig. 10.

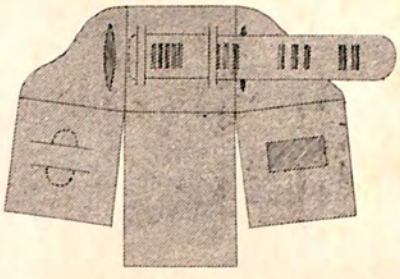

Fig. 14.

Fig. 11.

Fig. 12.

Fig. 13.

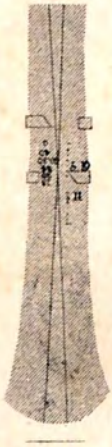

Fig. 15.

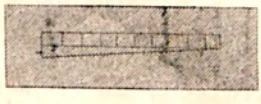

Fig. 16.

Fig. 17.

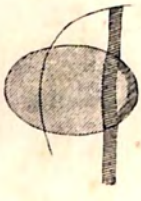

Fig. 18.

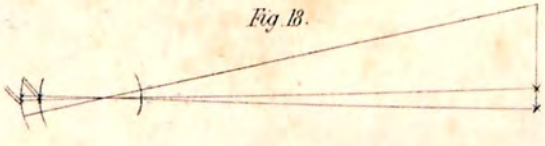

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Fig. 19.

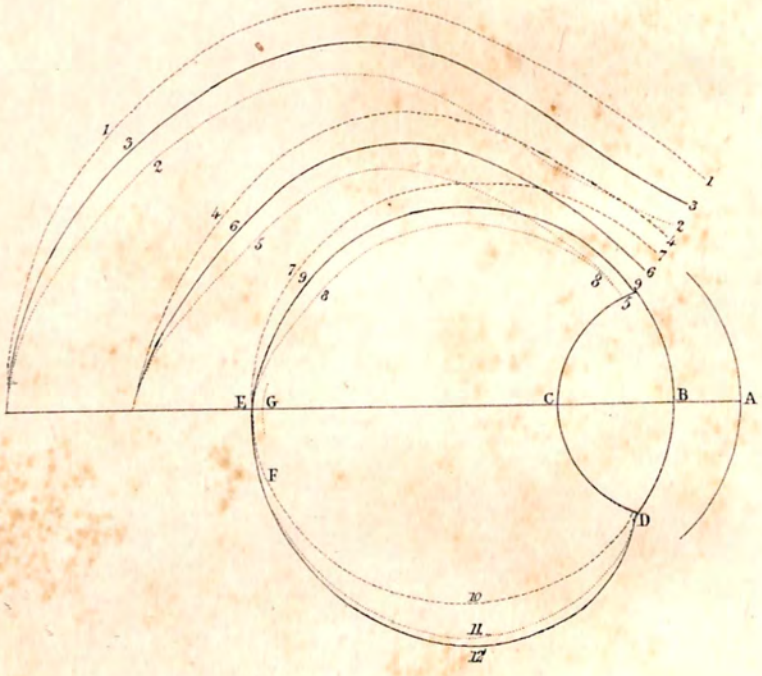

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Fig. 20

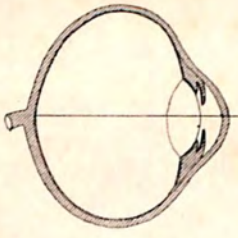
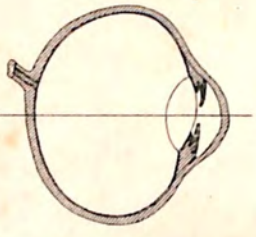


Fig. 21.



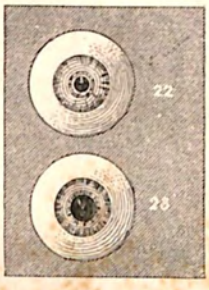


Fig. 24.

Fig. 25.

Fig. 28.

Fig. 26.

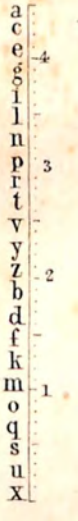


Fig. 27.

Fig. 30.

Fig. 29.

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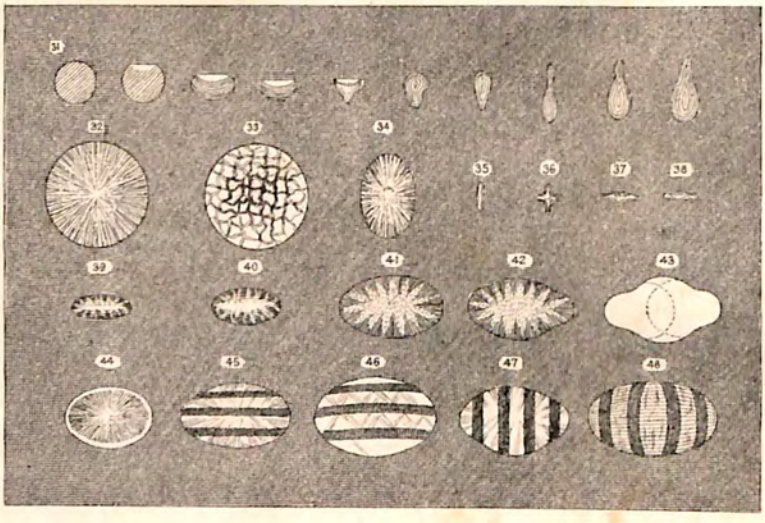

Fig. 49. Fig. 50. Fig. 51

Fig. 52. Fig. 53. Fig. 54.

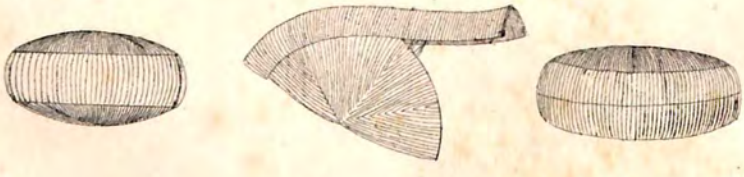

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Fig. 55.

Fig. 56.

Fig. 57.

Fig. 58.

Fig. 59.

Fig. 60.

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principles suggested by Dr. Porterfield; and to confirm his opinion of the utter inability of such persons to change the refractive state of the organ. Eighthly, to deduce, from the aberration of the lateral rays, a decisive argument in favour of a change in the figure of the crystalline; to ascertain, from the quantity of this aberration, the form into which the lens appears to be thrown in my own eye, and the mode by which the change must be produced in that of every other person. And I flatter myself that I shall not be deemed too precipitate in denominating this series of experiments satisfactorily demonstrative.

Explanation of the Figures.

Fig. 4. See page 16. Prop. III.

Fig. 5. See page 16. Prop. IV.

Fig. 6. See page 19. Prop. V.

Fig. 7—9. Relating to the optometer. See page 21.

Fig. 10. The form of the ends of the optometer, when made of card. The apertures in the shoulders are for holding a lens: the square ends turn under, and are fastened together.

Fig. 11. The scale of the optometer. The middle line is divided, from the lower end, into inches. The next column shows the number of a concave lens requisite for a short-sighted eye; by looking through the slider and observing the number opposite to which the intersection appears when most remote. By observing the place of apparent intersection when nearest, the number requisite will be found in the other column, provided that the eye have the average power of accommodation. At the other end, the middle line is graduated for extending the scale of inches by means of a lens four inches in focus; the negative numbers implying that such rays as proceed from them are made to converge towards a point on the other side of the lens. The other column shows the focal length of convex glasses required by those eyes to which the intersection appears, when nearest, opposite the respective places of the numbers.

Fig. 12. A side view of the optometer, half its size.

Fig. 13. The appearance of the lines through the slider.

Fig. 14. Method of measuring the magnitude of an image on the retina. See page 33.

Fig. 15. Diagonal scale drawn on a looking-glass.

Fig. 16. The method of applying a lens with water to the cornea.

Fig. 17. The appearance of a spectrum occasioned by pressure; and the inflection of straight lines seen within the limits of the spectrum.

Fig. 18. An illustration of the enlargement of the image, which would be the consequence of an elongation of the eye: the images of the candles, which, in one instance, fall on the insertion of the nerve, falling, in the other instance, beyond it.

Fig. 19. The successive forms of the image of a large distant object, as it would be delineated by each refractive surface in the eye; to show how that form at last coincides with the retina. EG is the distance between the foci of horizontal and vertical rays in my eye.

Fig. 20. Vertical section of my right eye, seen from without; twice the natural size.

Fig. 21. Horizontal section, seen from above.

Fig. 22. Front view of my left eye when the pupil is contracted; of the natural size.

Fig. 23. The same view when the pupil is dilated.

Fig. 24. Outline of the eye and its straight muscles when at rest.

Fig. 25. Change of figure which would be the consequence of the action of those muscles upon the eye, and upon the adipose substance behind it.

Fig. 26. Scale of the small optometer.

Fig. 27. Appearance of four images of a line seen by my eye when its focus is shortest.

Fig. 28. Outline of the lens when relaxed; from a comparison of M. Petit's measures with the phenomena of my own eye, and on the supposition that it is found in a relaxed state after death.

Fig. 29. Outline of the lens sufficiently changed to produce the shortest focal distance.

Fig. 30. Apparatus for ascertaining the focal length of the lens in water.

Fig. 31. Various forms of the image depicted by a cylindrical pencil of rays obliquely refracted by a spherical surface, when received on planes at distances progressively greater.

Fig. 32. Image of a minute lucid object held very near to my eye.

Fig. 33. The same appearance when the eye has been rubbed.

Fig. 34—40. Different forms of the image of a lucid point at greater and greater distances; the most perfect focus being like Fig. 36, but much smaller.

Fig. 41. Image of a very remote point seen by my right eye.

Fig. 42. Image of a remote point seen by my left eye; being more

obtuse at one end, probably from a less obliquity of the posterior surface of the crystalline lens.

Fig. 43. Combination of two figures similar to the fifth variety of Fig. 31; to imitate Fig. 41.

Fig. 44. Appearance of a distant lucid point when the eye is adapted to a very near object.

Fig. 45, 47. Shadow of parallel wires in the image of a distant point when the eye is relaxed.

Fig. 46, 48. The same shadows rendered curved by a change in the figure of the crystalline lens.

Fig. 49. The order of the fibres of the human crystalline.

Fig. 50. The division of the nerves at the ciliary zone; the sclerotica being removed. One of the nerves of the uvea is seen passing forwards and subdividing. From the calf.

Fig. 51. Ramifications from the margin of the crystalline lens.

Fig. 52. The zone of the crystalline faintly seen through the capsule.

Fig. 53. The zone raised from its situation, with the ramifications passing through it into the lens.

Fig. 54. The zone of the crystalline detached.

Fig. 55. The crenated zone, and the globules regularly arranged on the crystalline of the partridge.

Fig. 56. The order of the fibres in the lens of birds and fishes.

Fig. 57. The segments of the capsule of the crystalline turned back, to show the detached ciliary processes. From the calf.

Fig. 58. Part of the choroid of the cod-fish, with its red substance. The central artery hangs loose from the insertion of the nerve.

Fig. 59. The membrane covering this substance internally, raised by the blow-pipe.

Fig. 60. The appearance of the red substance, after the removal of the membrane.

No. III.

OUTLINES OF EXPERIMENTS AND INQUIRIES RESPECTING

SOUND AND LIGHT.

From the Philosophical Transactions.

IN A LETTER ADDRESSED TO EDWARD WHITTAKER GREY, M.D., SEC. R.S.

READ JANUARY 16TH, 1800.

DEAR SIR,

IT has long been my intention to lay before the Royal Society a few observations on the subject of sound; and I have endeavoured to collect as much information, and to make as many experiments, connected with this inquiry, as circumstances enabled me to do; but the further I have proceeded, the more widely the prospect of what lay before me has been extended; and, as I find that the investigation, in all its magnitude, will occupy the leisure hours of some years, or perhaps of a life, I am determined, in the mean time, lest any unforeseen circumstances should prevent my continuing the pursuit, to submit to the Society some conclusions which I have already formed from the results of various experiments. Their subjects are, I. The measurement of the quantity of air discharged through an aperture. II. The determination of the direction and velocity of a stream of air proceeding from an orifice. III. Ocular evidence of the nature of sound. IV. The velocity of sound. V. Sonorous cavities. VI. The degree of divergence of sound. VII. The decay of sound. VIII. The harmonic sounds of pipes. IX. The vibrations of different elastic fluids. X. The analogy between light and sound. XI. The coalescence of musical sounds. XII. The frequency of vibrations constituting a given note. XIII. The vibrations of chords. XIV. The vibrations of rods and

plates. XV. The human voice. XVI. The temperament of musical intervals.

I.—*Of the Quantity of Air discharged through an Aperture.*

A piece of bladder was tied over the end of the tube of a large glass funnel, and punctured with a hot needle. The funnel was inverted in a vessel of water; and a gage, with a graduated glass tube, was so placed as to measure the pressure occasioned by the different levels of the surfaces of the water. As the air escaped through the puncture, it was supplied by a phial of known dimensions, at equal intervals of time; and, according to the frequency of this supply, the average height of the gage was such as is expressed in the first Table. It appears, that the quantity of air discharged by a given aperture was nearly in the subduplicate ratio of the pressure; and that the ratio of the expenditures by different apertures, with the same pressure, lay between the ratio of their diameters and that of their areas. The second, third, and fourth Tables show the result of similar experiments, made with some variations in the apparatus. It may be inferred, from comparing the experiments on a tube with those on a simple perforation, that the expenditure is increased, as in water, by the application of a short pipe.

Table I.

A	B	C
.00018	.25	3.9
.00018	.58	11.7
.00018	1.	15.6
.001	.045	7.8
.001	.2	15.6
.001	.7	31.2
.004	.35	46.8

Table II.

A	B	C
.07	1.	2000.
.07	2.	2900.

I. A is the area, in square inches, of an aperture nearly circular. B, the pressure in inches. C, the number of cubic inches discharged in one minute.

All numbers throughout this paper, where the contrary is not expressed, are to be understood of inches, linear, square, or cubic.

II. A is the area of the section of a tube about two inches long. B, the pressure. C, the quantity of air discharged in a minute, by estimation.

Table III.

A		C	D
.0064	1.15	.2	46.8
.0064	10.	.45	46.8
.0064	13.5	.35	31.2
.0064	13.5	.7	46.8

Table IV.

A	B	C
.003	.28	46.8

III. A is the area of the section of a tube. B, its length. C, the pressure. D, the discharge in a minute.

IV. A is the area of an oval aperture, formed by flattening a glass tube at the end: its diameters were .025 and .152. B, the pressure. C, the discharge.

II.—*Of the Direction and Velocity of a Stream of Air.*

An apparatus was contrived for measuring, by means of a water-gage communicating with a reservoir of air, the pressure by which a current was forced from the reservoir through a cylindrical tube; and the gage was so sensible, that, a regular blast being supplied from the lungs, it showed the slight variation produced by every pulsation of the heart. The current of air issuing from the tube was directed downwards, upon a white plate, on which a scale of equal parts was engraved, and which was thinly covered with a coloured liquid: the breadth of the surface of the plate laid bare was observed at different distances from the tube, and with different degrees of pressure, care being taken that the liquid should be so shallow as to yield to the slightest impression of air. The results are collected in Tables V. and VI., and are exhibited to the eye in Figs. 61—72. In order to measure, with greater certainty and precision, the velocity of every part of the current, a second cavity, furnished with a gage, was provided, and pieces perforated with apertures of different sizes were adapted to its orifice: the axis of the current was directed as accurately as possible to the centres of these apertures, and the results of the experiments, with various pressures and distances, are inserted in Tables VII., VIII., and IX. The velocity of a stream being, both according to the commonly received opinion and to the experiments already related, nearly in the subduplicate

ratio of the pressure occasioning it, it was inferred, that an equal pressure would be required to stop its progress, and that the velocity of the current, where it struck against the aperture, must be in the subduplicate ratio of the pressure marked by the gage. The ordinates of the curves in Figs. 73—83 were therefore taken reciprocally in the subduplicate ratio of the pressure marked by the second gage to that indicated by the first, at the various distances represented by the abscisses. Each figure represents a different degree of pressure in the first cavity. The curve nearest the axis is deduced from observations in which the aperture opposed to the tube was not greater than that of the tube itself; and shows what would be the diameter of the current, if the velocities of every one of its particles in the same circular section, including those of the contiguous air, which must have acquired as much motion as the current has lost, were equal among themselves. As the central particles must be supposed to be less impeded in their motion than the superficial ones, of course the smaller the aperture opposed to the centre of the current, the greater the velocity ought to come out, and the ordinate of the curve the smaller; but, where the aperture was not greater than that of the tube, the difference of the velocities at the same distance was scarcely perceptible. When the aperture was larger than that of the tube, if the distance was very small, of course the average velocity came out much smaller than that which was inferred from a smaller aperture: but, where the ordinate of the internal curve became nearly equal to this aperture, there was but little difference between the velocities indicated with different apertures. Indeed, in some cases, a larger aperture seemed to indicate a greater velocity; this might have arisen in some degree from the smaller aperture not having been exactly in the centre of the current; but there is greater reason to suppose, that it was occasioned by some resistance derived from the air returning between the sides of the aperture and the current entering it. Where this took place, the external curves, which are so constructed as that their ordinates are reciprocally in the subduplicate ratio of the pressure observed in the second cavity, with

apertures equal in semidiameter to their initial ordinate, approach, for a short distance, nearer to the axis than the internal curve: after this, they continue their course very near to this curve. Hence it appears that no observable part of the motion diverged beyond the limits of the solid which would be formed by the revolution of the internal curve, which is seldom inclined to the axis in an angle so great as ten degrees. A similar conclusion may be made, from observing the flame of a candle subjected to the action of a blowpipe: there is no divergency beyond the narrow limits of the current; the flame, on the contrary, is everywhere forced by the ambient air towards the current, to supply the place of that which it has carried away by its friction. The lateral communication of motion, very ingeniously and accurately observed in water by Professor Venturi, is exactly similar to the motion here shown to take place in air; and these experiments fully justify him in rejecting the tenacity of water as its cause: no doubt it arises from the relative situation of the particles of the fluid, in the line of the current, to that of the particles in the contiguous strata, which is such as naturally to lead to a communication of motion nearly in a parallel direction; and this may properly be termed friction. The lateral pressure, which urges the flame of a candle towards the stream of air from a blowpipe, is probably exactly similar to that pressure which causes the inflection of a current of air near an obstacle. Mark the dimple which a slender stream of air makes on the surface of water; bring a convex body into contact with the side of the stream, and the place of the dimple will immediately show that the current is inflected towards the body, and, if the body be at liberty to move in every direction, it will be urged towards the current, in the same manner as, in Venturi's experiments, a fluid was forced up a tube inserted into the side of a pipe through which water was flowing. A similar interposition of an obstacle in the course of the wind is probably often the cause of smoky chimneys. One circumstance was observed in these experiments, which it is extremely difficult to explain, and which yet leads to very important consequences: it may be made distinctly perceptible to the

eye, by forcing a current of smoke very gently through a fine tube. When the velocity is as small as possible, the stream proceeds for many inches without any observable dilatation; it then immediately diverges at a considerable angle into a cone, Fig. 84; and, at the point of divergency, there is an audible and even visible vibration. The blowpipe also affords a method of observing this phænomenon: as far as can be judged from the motion of the flame, the current seems to make something like a revolution in the surface of the cone, but this motion is too rapid to be distinctly discerned. When the pressure is increased, the apex of the cone approaches nearer to the orifice of the tube, Figs. 85, 86; but no degree of pressure seems materially to alter its divergency. The distance of the apex from the orifice is not proportional to the diameter of the current; it rather appears to be the greater the smaller the current, and is much better defined in a small current than in a large one. Its distance in one experiment is expressed in Table X., from observations on the surface of a liquid; in other experiments, its respective distances were sometimes considerably less with the same degrees of pressure. It may be inferred, from the numbers of Tables VII. and VIII., that in several instances a greater height of the first gage produced a less height of the second: this arose from the nearer approach of the apex of the cone to the orifice of the tube, the stream losing a greater portion of its velocity by this divergence than it gained by the increase of pressure. At first sight, the form of the current bears some resemblance to the *vena contracta* of a jet of water; but Venturi has observed, that in water an increase of pressure increases, instead of diminishing, the distance of the contracted section from the orifice. Is it not possible, that the facility with which some spiders are said to project their fine threads to a great distance, may depend upon the small degree of velocity with which they are thrown out, so that, like a minute current, meeting with little interruption from the neighbouring air, they easily continue their course for a considerable time?

Table V.

A	1.	2.	3.	3.8
B	C	C	C	C
1.	.1	.1	.1	
2.	.12	.12	.2	
3.	.17	.25	.3	
4.	.2	.4	.4	
5.	.25	.5		
6.	.30	.52		
7.	.35	.54	.5	
8.	.37	.56		
9.	.39	.58		
10.	.40	.6	.6	.5
15.		.7		
18.	.50			
20.				

V. The diameter of the tube .07. A is the distance of the liquid from the orifice. B, the pressure. C, the diameter of the surface of the liquid displaced.

Table VI.

A	1.	2.
B	C	C
1.	.1	.1
2.	.13	
3.	.2	.2
4.	.25	.3
6.	.3	.4
7.	.35	.5
10.	.35	.6
15.	.35	.7
20.	.35	.7

Table VII.

A	.5	
B	.06	.15
C	D	D
.1	.083	
.2	.16	
.3	.25	.1
.4	.35	
.5	.45	
.6	.53	.2
.7	.6	
.8		.3
1.	.5	
1.2	.4	.4
1.5	.6	
2.	.67	.55
4.	1.3	1.
8.		2.
9.	.3	
14.	.5	

VI. Diameter of the tube .1. A, B, and C, as in Table V.

VII. Diameter of the tube .06. A is the distance of the opposite aperture from the orifice of the tube. B, the diameter of the aperture. C, the pressure, indicated by the first gage. D, the height of the second gage.

Table VIII.

A	.5				1.				2.				4.			
B	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	5.	.06	.15	.3	.5
C	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D
.1	.05	.05			.03				.017							
.2	.1	.1			.12	.08	.02		.034							
.5	.2	.22			.1	.00			.00							
1.	.32	.36	.1		.17	.1	.1	.05	.04							
2.	.52	.6	.2		.28	.22	.21	.08	.07							
3.	.8	.9	.3		.4	.36	.32	.12	.12		.1	.1				
4.	1.1	1.2	.4		.58	.52	.42	.16	.18		.15	.14				
5.		1.5	.5		.8	.68	.52	.2	.23		.2	.18	.04		.04	.05
6.		1.7	.6		1.	.83	.63	.25	.3		.25	.22	.05		.05	.06
7.		1.9	.7		1.2	1.	.75	.3	.35		.3	.26	.06		.06	.07
8.		2.1	.8		1.5	1.2	.88	.34	.4		.34	.3	.07		.07	.07
9.		2.3	.9		1.7	1.4	1.	.37	.45		.37	.34	.08		.08	.08
10.		2.6	1.		1.9	1.6	1.1	.4	.5		.4	.37	.09		.09	.09

VIII. Diameter of the tube .1. A, B, C, and D, as in Table VII.

Table IX.

A	1.15				3.3			4.
B	.15	.3	.5	1.	.06	.15	1.	.06
C	D	D	D	D	D	D	D	D
.5	.1	.1	.1					
1.	.2	.2	.2					
2.	.4	.35	.34	.13	.1	.1	.125	
3.	.6	.5	.5	.2	.15	.15	.18	.1

Table X.

A	B
.4	6.
.8	3.
1.2	1.5
1.8	1.
2.	.5
4.	.0

IX. Diameter of the tube .3. A, B, C, and D, as in Table VII.

X. A is the pressure. B, the distance of the apex of the cone from the orifice of a tube .1 in diameter.

III.—*Ocular Evidence of the Nature of Sound.*

A tube about the tenth of an inch in diameter, with a lateral orifice half an inch from its end, filed rather deeper than the axis of the tube, Fig. 87, was inserted at the apex of a conical cavity containing about twenty cubic inches of air, and luted perfectly tight: by blowing through the tube a sound nearly in unison with the tenor C was produced. By gradually increasing the capacity of the cavity as far as several gallons, with the same mouth-piece, the sound, although faint, became more and more grave, till it was no longer a musical note. Even before this period a kind of trembling was distinguishable; and this, as the cavity was still further increased, was changed

into a succession of distinct puffs, like the sound produced by an explosion of air from the lips; as slow, in some instances, as 4 or 3 in a second. These were undoubtedly the single vibrations, which, when repeated with sufficient frequency, impress on the auditory nerve the sensation of a continued sound. On forcing a current of smoke through the tube, the vibratory motion of the stream, as it passed out at the lateral orifice, was evident to the eye: although, from various circumstances, the quantity and direction of its motion could not be subjected to exact mensuration. This species of sonorous cavity seems susceptible of but few harmonic sounds. It was observed that a faint blast produced a much greater frequency of vibrations than that which was appropriate to the cavity: a circumstance similar to this obtains also in large organ-pipes; but, several minute observations of this kind, although they might assist in forming a theory of the origin of vibrations, or in confirming such a theory drawn from other sources, yet, as they are not alone sufficient to afford any general conclusions, are omitted at present, for the sake of brevity.

IV.—*Of the Velocity of Sound.*

It has been demonstrated by M. De la Grange and others, that any impression whatever communicated to one particle of an elastic fluid, will be transmitted through that fluid with an uniform velocity, depending on the constitution of the fluid, without reference to any supposed laws of the continuation of that impression. Their theorem for ascertaining this velocity is the same as Newton has deduced from the hypothesis of a particular law of continuation: but it must be confessed, that the result differs somewhat too widely from experiment, to give as full confidence in the perfection of the theory. Corrected by the experiments of various observers, the velocity of any impression transmitted by the common air, may, at an average, be reckoned 1130 feet in a second.

V.—*Of sonorous Cavities.*

M. De la Grange has also demonstrated, that all impressions are reflected by an obstacle terminating an elastic fluid, with

the same velocity with which they arrived at that obstacle. When the walls of a passage, or of an unfurnished room, are smooth and perfectly parallel, any explosion, or a stamping with the foot, communicates an impression to the air, which is reflected from one wall to the other, and from the second again towards the ear, nearly in the same direction with the primitive impulse: this takes place as frequently in a second, as double the breadth of the passage is contained in 1130 feet; and the ear receives a perception of a musical sound, thus determined in its pitch by the breadth of the passage. On making the experiment, the result will be found accurately to agree with this explanation. If the sound is predetermined, and the frequency of vibrations such as that each pulse, when doubly reflected, may coincide with the subsequent pulse proceeding directly from the sounding body, the intensity of the sound will be much increased by the reflection; and also, in a less degree, if the reflected pulse coincides with the next but one, the next but two, or more, of the direct pulses. The appropriate notes of a room may readily be discovered by singing the scale in it; and they will be found to depend on the proportion of its length or breadth to 1130 feet. The sound of the stopped diapason pipes of an organ is produced in a manner somewhat similar to the note from an explosion in a passage; and that of its reed pipes to the resonance of the voice in a room: the length of the pipe in one case determining the sound, in the other increasing its strength. The frequency of the vibrations does not at all immediately depend on the diameter of the pipe. It must be confessed that much remains to be done in explaining the precise manner in which the vibration of the air in an organ-pipe is generated. M. Daniel Bernoulli has solved several difficult problems relating to the subject; yet some of his assumptions are not only gratuitous, but contrary to matter of fact.

VI.—*Of the Divergence of Sound.*

It has been generally asserted, chiefly on the authority of Newton, that if any sound be admitted through an aperture into a chamber, it will diverge from that aperture equally in all directions. The chief arguments in favour of this opinion are

deduced from considering the phænomena of the pressure of fluids, and the motion of waves excited in a pool of water. But the inference seems to be too hastily drawn: there is a very material difference between impulse and pressure; and, in the case of waves of water, the moving force at each point is the power of gravity, which, acting primarily in a perpendicular direction, is only secondarily converted into a horizontal force, in the direction of the progress of the waves, being at each step disposed to spread equally in every direction: but the impulse transmitted by an elastic fluid, acts primarily in the direction of its progress. It is well known, that if a person calls to another with a speaking trumpet, he points it towards the place where his hearer stands: and I am assured by a very respectable Member of the Royal Society, that the report of a cannon appears many times louder to a person towards whom it is fired, than to one placed in a contrary direction. It must have occurred to every one's observation, that a sound such as that of a mill, or a fall of water, has appeared much louder after turning a corner, when the house or other obstacle no longer intervened; and it has been already remarked by Euler, on this head, that we are not acquainted with any substance perfectly impervious to sound. Indeed, as M. Lambert has very truly asserted, the whole theory of the speaking trumpet, supported as it is by practical experience, would fall to the ground, if it were demonstrable that sound spreads equally in every direction. In windy weather it may often be observed, that the sound of a distant bell varies almost instantaneously in its strength, so as to appear at least twice as remote at one time as at another; an observation which has also occurred to another gentleman, who is uncommonly accurate in examining the phænomena of nature. Now, if sound diverged equally in all directions, the variation produced by the wind could never exceed one-tenth of the apparent distance; but, on the supposition of a motion nearly rectilinear, it may easily happen that a slight change in the direction of the wind may convey the sound, either directly or after reflection, in very different degrees of strength, to the same spot. From the experiments on the motion of a current of air, already related, it would be

expected that a sound, admitted at a considerable distance from its origin through an aperture, would proceed, with an almost imperceptible increase of divergence, in the same direction; for, the actual velocity of the particles of air, in the strongest sound, is incomparably less than that of the slowest of the currents in the experiments related, where the beginning of the conical divergence took place at the greatest distance. Dr. Matthew Young has objected, not without reason, to M. Hube, that the existence of a condensation will cause a divergence in sound: but a much greater degree of condensation must have existed in the currents described than in any sound. There is indeed one difference between a stream of air and a sound; that, in sound, the motions of different particles of air are not synchronous; but it is not demonstrable that this circumstance would affect the divergency of the motion, except at the instant of its commencement, and perhaps not even then in a material degree; for, in general, the motion is communicated with a very gradual increase of intensity. The subject, however, deserves a more particular investigation; and, in order to obtain a more solid foundation for the argument, it is proposed, as soon as circumstances permit, to institute a course of experiments for ascertaining as accurately as possible the different strength of a sound once projected in a given direction, at different distances from the axis of its motion.

VII.—*Of the Decay of Sound.*

Various opinions have been entertained respecting the decay of sound. M. De la Grange has published a calculation, by which its force is shown to decay nearly in the simple ratio of the distances; and M. Daniel Bernoulli's equations for the sounds of conical pipes lead to a similar conclusion. The same inference would follow from a completion of the reasoning of Dr. Helsham, Dr. Matthew Young, and Professor Venturi. It has been very elegantly demonstrated by Maclaurin, and may also be proved in a much more simple manner, that when motion is communicated through a series of elastic bodies increasing in magnitude, if the number of bodies be supposed infinitely great, and their difference infinitely small, the motion

of the last will be to that of the first in the subduplicate ratio of their respective magnitudes; and since, in the case of concentric spherical laminæ of air, the bulk increases in the duplicate ratio of the distance, the motion will in this case be directly, and the velocity inversely, as the distance. But, however true this may be of the first impulse, it will appear, by pursuing the calculation a little further, that every one of the elastic bodies, except the last, receives an impulse in a retrograde direction, which ultimately impedes the effect of the succeeding impulse, as much as a similar cause promoted that of the preceding one: and thus, as sound must be conceived to consist of an infinite number of impulses, the motion of the last lamina will be precisely equal to that of the first; and, as far as this mode of reasoning goes, sound must decay in the duplicate ratio of the distance. Hence it appears, that the proposal for adopting the logarithmic curve for the form of the speaking trumpet, was founded on fallacious reasoning. The calculation of M. De la Grange is left for future examination; and it is intended, in the mean time, to attempt to ascertain the decay of sound as nearly as possible by experiment: should the result favour the conclusions from that calculation, it would establish a marked difference between the propagation of sound and of light.

VIII.—*Of the Harmonic Sounds of Pipes.*

In order to ascertain the velocity with which organ-pipes of different lengths require to be supplied with air, according to the various appropriate sounds which they produce, a set of experiments was made, with the same mouthpiece, on pipes of the same bore, and of different lengths, both stopped and open. The general result was, that a similar blast produced as nearly the same sound as the length of the pipes would permit; or at least that the exceptions, though very numerous, lay equally on each side of this conclusion. The particular results are expressed in Table XI. and in Fig. 88. They explain how a note may be made much louder on a wind instrument by a swell, than it can possibly be by a sudden impression of the blast. It is proposed, at a future time, to ascertain, by experiment, the actual compression of the air within the pipe under different

circumstances: from some very slight trials, it seemed to be nearly in the ratio of the frequency of vibrations of each harmonic.

Table XI.

OPEN.						STOPPED.					
A	B	C	D	E	F	A	B	C	D	E	F
4.5		0.7	8.8	$\overline{\overline{d\sharp\sharp}}$	1	4.5		0.3	1·8	$\overline{\overline{d}}$	1
	4.1	8.8			2		1.2	1.7	10·0		3
							5.0	9.0			5
9.4		0.3	0.9	$\overline{f}$	1	9.4		0.2	0.4	$\overline{f}$	1
	0.8		8.0		2			0.45	1.6		3
	2.0		18.0		3		1.1	1.6	8.5		5
	5.0	8.0	20.0		4		7.0	8.0			7
	16.5	18.0			5						
	19.0	20.0			6						
16.1		0.4	1.0	$\overline{\overline{g\sharp\sharp}}$	2	16.1		0.4	0.6	$\overline{\overline{d\sharp\sharp}}$	3
	0.8	1.0	2.2		3		0.6	0.65	1.1		5
	1.2	2.2	4.7		4		0.9	1.1	2.4		7
	2.2	4.7	11.5		5		1.6	2.4	4.9		9
	3.4		13.5		6		2.5	4.8	9.0		11
	4.0		15.0		7		6.0	7.0			13
	6.5	10.0			8						
20.5		0.6	0.8	$\overline{\overline{b}}$	3	20.5		0.8	1.1	$\overline{\overline{c\sharp\sharp}}$	7
		0.8	1.9		4		1.0	1.1	3.8		9
	1.1	1.9	5.7		5		1.8		3.8		11
	4.5	5.7			8		3.2	3.8	12.		17
								12.		0	00

XI. A is the length of the pipe from the lateral orifice to the end. C, the pressure at which the sound began. B, its termination, by lessening the pressure; D, by increasing it. E, the note answering to the first sound of each pipe, according to the German method of notation. F, the number showing the place of each note in the regular series of harmonics. The diameter of the pipe was .35; the air-duct of the mouth-piece measured, where smallest, .25 by .035; the lateral orifice .25 by .125. The apparatus was not calculated to apply a pressure of above 22 inches. Where no number stands under C, a sudden blast was required to produce the note.

IX.—*Of the Vibrations of different Elastic Fluids.*

All the methods of finding the velocity of sound agree in determining it to be, in fluids of a given elasticity, reciprocally

in the subduplicate ratio of the density: hence, in pure hydrogen gas it should be $\sqrt{13} = 3.6$ times as great as in common air; and the pitch of a pipe should be a minor fourteenth higher in this fluid than in the common air. It is therefore probable that the hydrogen gas used in Professor Chladni's late experiments was not quite pure. It must be observed, that in an accurate experiment of this nature, the pressure causing the blast ought to be carefully ascertained. There can be no doubt but that, in the observations of the French Academicians on the velocity of sound, which appear to have been conducted with all possible attention, the dampness and coldness of the night air must have considerably increased its density: hence, the velocity was found to be only 1109 feet in a second; while Derham's experiments, which have an equal appearance of accuracy, make it amount to 1142. Perhaps the average may, as has been already mentioned, be safely estimated at 1130. It may be here remarked, that the well-known elevation of the pitch of wind instruments, in the course of playing, sometimes amounting to half a note, is not, as is commonly supposed, owing to any expansion of the instrument, for this should produce a contrary effect, but to the increased warmth of the air in the tube. Dr. Smith has made a similar observation, on the pitch of an organ in summer and winter, which he found to differ more than twice as much as the English and French experiments on the velocity of sound. Bianconi found the velocity of sound, at Bologna, to differ, at different times, in the ratio of 152 to 157.

X.—*Of the Analogy between Light and Sound.*

Ever since the publication of Sir Isaac Newton's incomparable writings, his doctrines of the emanation of particles of light from lucid substances, and of the formal pre-existence of coloured rays in white light, have been almost universally admitted in this country, and but little opposed in others. Leonard Euler indeed, in several of his works, has advanced some powerful objections against them, but not sufficiently powerful to justify the dogmatical reprobation with which he treats them; and he has left that system of an ethereal vibration,

which after Huygens and some others he adopted, equally liable to be attacked on many weak sides. Without pretending to decide positively on the controversy, it is conceived that some considerations may be brought forwards, which may tend to diminish the weight of objections to a theory similar to the Huygenian. There are also one or two difficulties in the Newtonian system, which have been little observed. The first is, the uniform velocity with which light is supposed to be projected from all luminous bodies, in consequence of heat, or otherwise. How happens it that, whether the projecting force is the slightest transmission of electricity, the friction of two pebbles, the lowest degree of visible ignition, the white heat of a wind furnace, or the intense heat of the sun itself, these wonderful corpuscles are always propelled with one uniform velocity? For, if they differed in velocity, that difference ought to produce a different refraction. But a still more insuperable difficulty seems to occur, in the partial reflection from every refracting surface. Why, of the same kind of rays, in every circumstance precisely similar, some should always be reflected, and others transmitted, appears in this system to be wholly inexplicable. That a medium resembling, in many properties, that which has been denominated ether, does really exist, is undeniably proved by the phænomena of electricity; and the arguments against the existence of such an ether throughout the universe, have been pretty sufficiently answered by Euler. The rapid transmission of the electrical shock shows that the electric medium is possessed of an elasticity as great as is necessary to be supposed for the propagation of light. Whether the electric ether is to be considered as the same with the luminous ether, if such a fluid exists, may perhaps at some future time be discovered by experiment; hitherto I have not been able to observe that the refractive power of a fluid undergoes any change by electricity. The uniformity of the motion of light in the same medium, which is a difficulty in the Newtonian theory, favours the admission of the Huygenian; as all impressions are known to be transmitted through an elastic fluid with the same velocity. It has been already shown, that sound, in all probability, has very little tendency to diverge: in a medium

so highly elastic as the luminous ether must be supposed to be, the tendency to diverge may be considered as infinitely small, and the grand objection to the system of vibration will be removed. It is not absolutely certain, that the white line visible in all directions on the edge of a knife, in the experiments of Newton and of Mr. Jordan, was not partly occasioned by the tendency of light to diverge. Euler's hypothesis, of the transmission of light by an agitation of the particles of the refracting media themselves, is liable to strong objections; according to this supposition, the refraction of the rays of light, on entering the atmosphere from the pure ether which he describes, ought to be a million times greater than it is. For explaining the phænomena of partial and total reflection, refraction, and inflection, nothing more is necessary than to suppose all refracting media to retain, by their attraction, a greater or less quantity of the luminous ether, so as to make its density greater than that which it possesses in a vacuum, without increasing its elasticity; and that light is a propagation of an impulse communicated to this ether by luminous bodies: whether this impulse is produced by a partial emanation of the ether, or by vibrations of the particles of the body, and whether these vibrations are, as Euler supposed, of various and irregular magnitudes, or whether they are uniform, and comparatively large, remains to be hereafter determined. Now, as the direction of an impulse, transmitted through a fluid, depends on that of the particles in synchronous motion, to which it is always perpendicular, whatever alters the direction of the pulse, will inflect the ray of light. If a smaller elastic body strike against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body, striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays

of light pass from a denser to a rarer stratum. Let AB, Fig. 89, be a ray of light falling on the reflecting surface FG; *cd* the direction of the vibration, pulse, impression, or condensation. When *d* comes to H, the impression will be, either wholly or partly, reflected with the same velocity as it arrived, and EH will be equal to DH; the angle EIH to DIH or CIF; and the angle of reflection to that of incidence. Let FG, Fig. 90, be a refracting surface. The portion of the pulse IE, which is travelling through the refracting medium, will move with a greater or less velocity in the subduplicate ratio of the densities, and HE will be to KI in that ratio. But HE is, to the radius IH, the sine of the angle of refraction; and KI that of the angle of incidence. This explanation of refraction is nearly the same as that of Euler. The total reflection of a ray of light by a refracting surface is explicable in the same manner as its simple refraction; HE, Fig. 91, being so much longer than KI, that the ray first becomes parallel to FG, and then, having to return through an equal diversity of media, is reflected in an equal angle. When a ray of light passes near an inflecting body, surrounded, as all bodies are supposed to be, with an atmosphere of ether denser than the ether of the ambient air, the part of the ray nearest the body is retarded, and of course the whole ray inflected towards the body, Fig. 92. The repulsion of inflected rays has been very ably controverted by Mr. Jordan, the ingenious author of a late publication on the Inflection of Light. It has already been conjectured by Euler, that the colours of light consist in the different frequency of the vibrations of the luminous ether: it does not appear that he has supported this opinion by any argument; but it is strongly confirmed, by the analogy between the colours of a thin plate and the sounds of a series of organ-pipes.* The phænomena of the colours of thin plates require, in the Newtonian system, a very complicated supposition, of an ether, anticipating by its motion the velocity of the corpuscles of light, and thus producing the fits of transmission and reflection; and even this supposition does not much assist the explanation. It appears

* This analogy is fanciful and altogether unfounded.—*Note by the Editor.*

from the accurate analysis of the phænomena which Newton has given, and which has by no means been superseded by any later observations, that the same colour recurs whenever the thickness answers to the terms of an arithmetical progression. Now this is precisely similar to the production of the same sound, by means of an uniform blast, from organ-pipes which are different multiples of the same length. Supposing white light to be a continued impulse or stream of luminous ether, it may be conceived to act on the plates as a blast of air does on the organ-pipes, and to produce vibrations regulated in frequency by the length of the lines which are terminated by the two refracting surfaces. It may be objected that, to complete the analogy, there should be tubes to answer to the organ-pipes: but the tube of an organ-pipe is only necessary to prevent the divergence of the impression, and in light there is little or no tendency to diverge; and indeed, in the case of a resonant passage, the air is not prevented from becoming sonorous by the liberty of lateral motion. It would seem that the determination of a portion of the track of a ray of light through any homogeneous stratum of ether is sufficient to establish a length as a basis for colorific vibrations. In inflections the length of the track of a ray of light through the inflecting atmosphere may determine its vibrations: but, in this case, as it is probable that there is a reflection from every part of the surface of the surrounding atmosphere, contributing to the appearance of the white line in every direction, in the experiments already mentioned, so it is possible that there may be some second reflection at the immediate surface of the body itself, and that, by mutual reflections between these two surfaces, something like the anguiform motion suspected by Newton may really take place; and then the analogy to the colours of thin plates will be still stronger. A mixture of vibrations, of all possible frequencies, may easily destroy the peculiar nature of each, and concur in a general effect of white light. The greatest difficulty in this system is, to explain the different degree of refraction of differently coloured light, and the separation of white light in refraction; yet, considering how imperfect the theory of elastic fluids still remains, it cannot be expected that every circumstance should

at once be clearly elucidated. It may hereafter be considered how far the excellent experiments of Count Rumford, which tend very greatly to weaken the evidence of the modern doctrine of heat, may be more or less favourable to one or the other system of light and colours. It does not appear that any comparative experiments have been made on the inflection of light by substances possessed of different refractive powers; undoubtedly some very interesting conclusions might be expected from the inquiry.

XI.—*Of the Coalescence of Musical Sounds.*

It is surprising that so great a mathematician as Dr. Smith could have entertained for a moment, an idea that the vibrations constituting different sounds should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise effected than by each particle's partaking of both motions. If this assertion stood in need of any proof, it might be amply furnished by the phænomena of beats, and of the grave harmonics observed by Romieu and Tartini; which M. De la Grange has already considered in the same point of view. In the first place, to simplify the statement, let us suppose, what probably never precisely happens, that the particles of air, in transmitting the pulses, proceed and return with uniform motions; and in order to represent their position to the eye, let the uniform progress of time be represented by the increase of the abscis, and the distance of the particle from its original position, by the ordinate, Fig. 93—98. Then, by supposing any two or more vibrations in the same direction to be combined, the joint motion will be represented by the sum or difference of the ordinates. When two sounds are of equal strength, and nearly of the same pitch, as in Fig. 96, the joint vibration is alternately very weak and very strong, producing the effect denominated a beat, Fig. 103, B and C; which is slower and more marked, as the sounds approach nearer to each other in frequency of vibrations; and, of these beats there may happen to be several orders, according to the

periodical approximations of the numbers expressing the proportions of the vibrations. The strength of the joint sound is double that of the simple sound only at the middle of the beat, but not throughout its duration; and it may be inferred, that the strength of sound in a concert will not be in exact proportion to the number of instruments composing it. Could any method be devised for ascertaining this by experiment, it would assist in the comparison of sound with light. In Fig. 93, let P and Q be the middle points of the progress or regress of a particle in two successive compound vibrations; then, CP being = PD, KR = RN, GQ = QH, and MS = SO, twice their distance, 2RS = 2RN + 2NM + 2MS = KN + NM + NM + MO = KM + NO, is equal to the sum of the distances of the corresponding parts of the simple vibrations. For instance, if the two sounds be as 80 : 81, the joint vibration will be as 80.5; the arithmetical mean between the periods of the single vibrations. The greater the difference in the pitch of two sounds, the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related, they communicate the idea of a continued sound; and this is the fundamental harmonic described by Tartini. For instance, in Fig. 94—97, the vibrations of sounds related as 1 : 2, 4 : 5, 9 : 10, and 5 : 8, are represented; where the beats, if the sounds be not taken too grave, constitute a distinct sound, which corresponds with the time elapsing between two successive coincidences, or near approaches to coincidence: for, that such a tempered interval still produces a harmonic, appears from Fig. 98. But, besides this primary harmonic, a secondary note is sometimes heard, where the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of two sounds related to each other as 7 : 8, 5 : 7, or 4 : 5, there is a recurrence of a similar state of the joint motion, nearly at the interval of $\frac{5}{12}$, $\frac{4}{9}$, or $\frac{3}{9}$ of the whole period: hence in the concord of a major third, the fourth below the key note is heard as distinctly as the double octave, as is seen in some degree in Fig. 95; AB being nearly two-thirds of CD. The same sound is sometimes produced by taking the minor sixth below the key note; probably

because this sixth, like every other note, is almost always attended by an octave, as a harmonic. If the angles of all the figures resulting from the motion thus assumed be rounded off, they will approach more nearly to a representation of the actual circumstances; but, as the laws by which the motion of the particles of air is regulated, differ according to the different origin and nature of the sound, it is impossible to adapt a demonstration to them all: if, however, the particles be supposed to follow the law of the harmonic curve, derived from uniform circular motion, the compound vibration will be the harmonic instead of the arithmetical mean; and the secondary sound of the interrupted vibrations will be more accurately formed, and more strongly marked, Figs. 101, 102: the demonstration is deducible from the properties of the circle. It is remarkable, that the law by which the motion of the particles is governed, is capable of some singular alterations by a combination of vibrations. By adding to a given sound other similar sounds, related to it in frequency, as the series of odd numbers, and in strength inversely in the same ratios, the right lines indicating an uniform motion may be converted very nearly into figures of sines, and the figures of sines into right lines, as in Figs. 99, 100.

XII.—*Of the Frequency of Vibrations constituting a given Note.*

The number of vibrations performed by a given sound in a second, has been variously ascertained; first, by Sauveur, by a very ingenious inference from the beats of two sounds; and since, by the same observer and several others, by calculation from the weight and tension of a chord. It was thought worth while, as a confirmation, to make an experiment suggested, but coarsely conducted, by Mersennus, on a chord 200 inches in length, stretched so loosely as to have its single vibrations visible; and, by holding a quill nearly in contact with the chord, they were made audible, and were found, in one experiment, to recur 8.3 times in a second. By lightly pressing the chord at one-eighth of its length from the end, and at other shorter aliquot distances, the fundamental note was found to be

one-sixth of a tone higher than the respective octave of a tuning-fork marked C: hence, the fork was a comma and a half above the pitch assumed by Sauveur, of an imaginary C, consisting of one vibration in a second.

XIII.—*Of the Vibrations of Chords.*

By a singular oversight in the demonstration of Dr. Brook Taylor, adopted as it has been by a number of later authors, it is asserted, that if a chord be once inflected into any other form than that of the harmonic curve, it will, since those parts which are without this figure are impelled towards it by an excess of force, and those within it by a deficiency, in a very short time arrive at or very near the form of this precise curve. It would be easy to prove, if this reasoning were allowed, that the form of the curve can be no other than that of the axis, since the tending force is continually impelling the chord towards this line. The case is very similar to that of the Newtonian proposition respecting sound. It may be proved, that every impulse is communicated along a tended chord with an uniform velocity; and this velocity is the same which is inferred from Dr. Taylor's theorem; just as that of sound, determined by other methods, coincides with the Newtonian result. But, although several late mathematicians have given admirable solutions of all possible cases of the problem, yet it has still been supposed, that the distinctions were too minute to be actually observed; especially, as it might have been added, since the inflexibility of a wire would dispose it, according to the doctrine of elastic rods, to assume the form of the harmonic curve. The theorem of Euler and De la Grange, in the case where the chord is supposed to be at first at rest, is in effect this: continue the figure each way, alternately on different sides of the axis, and in contrary positions; then, from any point of the curve, take an abscis each way, in the same proportion to the length of the chord as any given portion of time bears to the time of one semivibration, and the half sum of the ordinates will be the distance of that point of the chord from the axis, at the expiration of the time given. If the initial figure of the chord be composed of two right lines, as generally hap-

pens in musical instruments and experiments, its successive forms will be such as are represented in Figs. 107, 108: and this result is fully confirmed by experiment. Take one of the lowest strings of a square piano forte, round which a fine silvered wire is wound in a spiral form: contract the light of a window, so that, when the eye is placed in a proper position, the image of the light may appear small, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path, like a burning coal whirled round, and will present to the eye a line of light, which by the assistance of a microscope, may be very accurately observed. According to the different ways by which the wire is put in motion, the form of this path is no less diversified and amusing, than the multifarious forms of the quiescent lines of vibrating plates, discovered by Professor Chladni; and is indeed in one respect even more interesting, as it appears to be more within the reach of mathematical calculation to determine it; although hitherto, excepting some slight observations of Busse and Chladni, principally on the motion of rods, nothing has been attempted on the subject. For the present purpose, the motion of the chord may be simplified, by tying a long fine thread to any part of it, and fixing this thread in a direction perpendicular to that of the chord, without drawing it so tight as to increase the tension: by these means, the vibrations are confined nearly to one plane, which scarcely ever happens when the chord vibrates at liberty. If the chord be now inflected in the middle, it will be found, by comparison with an object which marked its quiescent position, to make equal excursions on each side of the axis; and the figure which it apparently occupies will be terminated by two lines, the more luminous as they are nearer the ends, Fig. 109. But, if the chord be inflected near one of its extremities, Fig. 110, it will proceed but a very small distance on the opposite side of the axis, and will there form a very bright line, indicating its longer continuance in that place; yet it will return on the former side nearly to the point from whence it was let go, but will be there very faintly visible, on account of its short delay. In the middle of the

chord, the excursions on each side the axis are always equal; and, beyond the middle, the same circumstances take place as in the half where it was inflected, but on the opposite side of the axis; and this appearance continues unaltered in its proportions, as long as the chord vibrates at all: fully confirming the non-existence of the harmonic curve, and the accuracy of the construction of Euler and De la Grange. At the same time, as M. Bernoulli has justly observed, since every figure may be infinitely approximated, by considering its ordinates as composed of the ordinates of an infinite number of trochoids of different magnitudes, it may be demonstrated that all these constituent curves would revert to their initial state, in the same time that a similar chord bent into a trochoidal curve would perform a single vibration; and this is in some respects a convenient and compendious method of considering the problem. But, when a chord vibrates freely, it never remains long in motion, without a very evident departure from the plane of the vibration; and, whether from the original obliquity of the impulse, or from an interference with the reflected vibrations of the air, or from the inequability of its own weight or flexibility, or from the immediate resistance of the particles of air in contact with it, it is thrown into a very evident rotatory motion, more or less simple and uniform according to circumstances. Some specimens of the figures of the orbits of chords are exhibited in Fig. 104. At the middle of the chord, its orbit has always two equal halves, but seldom at any other point. The curves of Fig. 106 are described by combining together various circular motions, supposed to be performed in aliquot parts of the primitive orbit: and some of them approach nearly to the figures actually observed. When the chord is of unequal thickness, or when it is loosely tended and forcibly inflected, the apsides and double points of the orbits have a very evident rotatory motion. The compound rotations seem to demonstrate to the eye the existence of secondary vibrations, and to account for the acute harmonic sounds which generally attend the fundamental sound. There is one fact respecting these secondary notes, which seems entirely to have escaped observation. If a chord be inflected at one-half, one-third, or

any other aliquot part of its length, and then suddenly left at liberty, the harmonic note which would be produced by dividing the chord at that point is entirely lost, and is not to be distinguished during any part of the continuance of the sound. This demonstrates, that the secondary notes do not depend upon any interference of the vibrations of the air with each other, nor upon any sympathetic agitation of auditory fibres, nor upon any effect of reflected sound upon the chord, but merely upon its initial figure and motion. If it were supposed that the chord when inflected into right lines, resolved itself necessarily into a number of secondary vibrations, according to some curves which, when properly combined, would approximate to the figure given, the supposition would indeed in some respects correspond with the phænomenon related; as the coefficients of all the curves supposed to end at the angle of inflection would vanish. But, whether we trace the constituent curves of such a figure through the various stages of their vibrations, or whether we follow the more compendious method of Euler to the same purpose, the figures resulting from this series of vibrations are in fact so simple, that it seems inconceivable how the ear should deduce the complicated idea of a number of heterogeneous vibrations, from a motion of the particles of air which must be extremely regular, and almost uniform; an uniformity which, when proper precautions are taken, is not contradicted by examining the motion of the chord with the assistance of a powerful magnifier. This difficulty occurred very strongly to Euler; and De la Grange even suspects some fallacy in the experiment, and that a musical ear judges from previous association. But, besides that these sounds are discoverable to an ear destitute of such associations, and, when the sound is produced by two strings in imperfect unison, may be verified by counting the number of their beats, the experiment already related is an undeniable proof that no fallacy of this kind exists. It must be confessed, that nothing fully satisfactory has yet occurred to account for the phænomena: but it is highly probable that the slight increase of tension produced by flexure, which is omitted in the calculations, and the unavoidable inequality of thickness or flexibility of different

parts of the same chord, may, by disturbing the isochronism of the subordinate vibrations, cause all that variety of sounds which is so inexplicable without them. For, when the slightest difference is introduced in the periods, there is no difficulty in conceiving how the sounds may be distinguished; and indeed, in some cases, a nice ear will discover a slight imperfection in the tune of harmonic notes: it is also often observed, in tuning an instrument, that some of the single chords produce beating sounds, which undoubtedly arise from their want of perfect uniformity. It may be perceived that any particular harmonic is loudest, when the chord is inflected at about one-third of the corresponding aliquot part from one of the extremities of that part. An observation of Dr. Wallis seems to have passed unnoticed by later writers on harmonics. If the string of a violin be struck in the middle, or at any other aliquot part, it will give either no sound at all, or a very obscure one. This is true, not of inflection, but of the motion communicated by a bow; and may be explained from the circumstance of the successive impulses, reflected from the fixed points at each end, destroying each other: an explanation nearly analogous to some observations of Dr. Matthew Young on the motion of chords. When the bow is applied not exactly at the aliquot point, but very near it, the corresponding harmonic is extremely loud; and the fundamental note, especially in the lowest harmonics, scarcely audible: the chord assumes the appearance, at the aliquot points, of as many lucid lines as correspond to the number of the harmonic, more nearly approaching to each other as the bow approaches more nearly to the point, Fig. 111. According to the various modes of applying the bow, an immense variety of figures of the orbits are produced, Fig. 105, more than enough to account for all the difference of tone in different performers. In observations of this kind, a series of harmonics is frequently heard in drawing the bow across the same part of the chord: these are produced by the bow; they are however not proportionate to the whole length of the bow, but depend on the capability of the portion of the bowstring, intercepted between its end and the chord, of performing its vibrations in times which are aliquot parts of the

vibration of the chord: hence it would seem, that the bow takes effect on the chord but at one instant during each fundamental vibration. In these experiments, the bow was strung with the second string of a violin: and, in the preparatory application of resin, the longitudinal sound of Chladni was sometimes heard; but it was observed to differ at least a note in different parts of the string.

XIV.—*Of the Vibrations of Rods and Plates.*

Some experiments were made, with the assistance of a most excellent practical musician, on the various notes produced by a glass tube, an iron rod, and a wooden ruler; and, in a case where the tube was as much at liberty as possible, all the harmonics corresponding to the numbers from 1 to 13, were distinctly observed; several of them at the same time, and others by means of different blows. This result seems to differ from the calculations of Euler and Count Riccati, confirmed as they are by the repeated experiments of Professor Chladni; it is not therefore brought forward as sufficiently controverting those calculations, but as showing the necessity of a revision of the experiments. Scarcely any note could ever be heard when a rod was loosely held at its extremity; nor when it was held in the middle, and struck one-seventh of the length from one end. The very ingenious method of Professor Chladni, of observing the vibrations of plates by strewing fine sand over them, and discovering the quiescent lines by the figures into which it is thrown, has hitherto been little known in this country; his treatise on the phænomena is so complete, that no other experiments of the kind were thought necessary. Glass vessels of various descriptions, whether made to sound by percussion or friction, were found to be almost entirely free from harmonic notes; and this observation coincides with the experiments of Chladni.

XV.—*Of the Human Voice.*

The human voice, which was the object originally proposed to be illustrated by these researches, is of so complicated a nature, and so imperfectly understood, that it can be on this

occasion but superficially considered. No person, unless we except M. Ferrein, has published anything very important on the subject of the formation of the voice, before or since Dodart; his reasoning has fully shown the analogy between the voice and the *voix humaine* and regal organ-pipes; but his comparison with the whistle is unfortunate; nor is he more happy in his account of the falsetto. A kind of experimental analysis of the voice may be thus exhibited. By drawing in the breath, and at the same time properly contracting the larynx, a slow vibration of the ligaments of the glottis may be produced, making a distinct clicking sound: upon increasing the tension, and the velocity of the breath, this clicking is lost, and the sound becomes continuous, but of an extremely grave pitch: it may, by a good ear, be distinguished two octaves below the lowest A of a common bass voice, consisting in that case of about 26 vibrations in a second. The same sound may be raised nearly to the pitch of the common voice; but it is never smooth and clear, except perhaps in some of those persons called ventriloquists. When the pitch is raised still higher, the upper orifice of the larynx, formed by the summits of the arytænoid cartilages and the epiglottis, seems to succeed to the office of the ligaments of the glottis, and to produce a retrograde falsetto, which is capable of a very great degree of acuteness. The same difference probably takes place between the natural voice and the common falsetto: the rimula glottidis being too long to admit of a sufficient degree of tension for very acute sounds, the upper orifice of the larynx supplies its place; hence, taking a note within the compass of either voice, it may be held, with the same expanse of air, two or three times as long in a falsetto as in a natural voice; hence, too, the difficulty of passing smoothly from the one voice to the other. It has been remarked that the larynx is always elevated when the sound is acute: but this elevation is only necessary in rapid transitions, as in a shake; and then probably because, by the contraction of the capacity of the trachea, an increase of the pressure of the breath can be more rapidly effected this way, than by the action of the abdominal muscles alone. The reflection of the sound thus produced from the various parts of the cavity of the mouth and nostrils,

mixing at various intervals with the portions of the vibrations directly proceeding from the larynx, must, according to the temporary form of the parts, variously affect the laws of the motion of the air in each vibration, or, according to Euler's expression, the equation of the curve conceived to correspond with this motion, and thus produce the various characters of the vowels and semi-vowels. The principal sounding board seems to be the bony palate: the nose, except in nasal letters, affords but little resonance; for the nasal passage may be closed by applying the finger to the soft palate, without much altering the sound of vowels not nasal. A good ear may distinctly observe, especially in a loud bass voice, besides the fundamental note, at least four harmonic sounds, in the order of the natural numbers; and, the more reedy the tone of the voice, the more easily they are heard. Faint as they are, their origin is by no means easy to be explained. This observation is precisely confirmed in a late dissertation of M. Knecht, published in the musical newspaper of Leipsic. Perhaps, by a close attention to the harmonics entering into the constitution of various sounds, more may be done in their analysis than could otherwise be expected.

XVI.—*Of the Temperament of Musical Intervals.*

It would have been extremely convenient for practical musicians, and would have saved many warm controversies among theoretical ones, if three times the ratio of 4 to 5, or four times that of 5 to 6, had been equal to the ratio of 1 to 2. As it happens to be otherwise, it has been much disputed in what intervals the imperfection should be placed. The Aristoxenians and Pythagoreans were in some sense the beginners of the controversy. Sauveur has given very comprehensive tables of a great number of systems of temperament; and his own now ranks among the many that are rejected. Dr. Smith* has written a large and obscure volume, which, for every purpose but for the use of an impracticable instrument, leaves the whole subject precisely where it found it. Kirnberger, Marpurg, and other German writers, have disputed with great

* See infra, p. 134.

bitterness, almost every one for a particular method of tuning. It is not with any confidence of success that one more attempt is made, which rests its chief claim to preference on the similarity of its theory to the actual practice of the best instrument-makers. However we estimate the degree of imperfection of two tempered concords of the same nature, it will appear that the manner of dividing the temperament between them does not materially alter its aggregate sum; for instance, the imperfection of a comma, in a major third, occasions it to beat very nearly twice as fast as that of half a comma. If, indeed, the imperfection were great, it might affect an interval so materially as to destroy its character; as, in some methods of temperament, a minor third diminished by two commas approaches more nearly to the ratio 6 : 7, than to 5 : 6; but, with this limitation, the sum of harmony is nearly equal in all systems. Hence, if every one of the twelve major and minor thirds occurred equally often in the compositions which are to be performed on an instrument, it would be of no great consequence, to the sum of the imperfections, among which of the thirds they were divided: and, even in this case, the opinion of the best practical authors is, that the difference of character produced by a difference of proportions in various keys, would be of considerable advantage in the general effect of modulation. But, when it is considered, that upon an average, of all the music ever composed, some particular keys occur at least twice as often as others, there seems to be a very strong additional reason for making the harmony the most perfect in those keys which are the most frequently used; since the aggregate sum of all the imperfections which occur in playing, must by this means be diminished in the greatest possible degree, and the diversity of character at the same time preserved. Indeed, in practice, this method, under different modifications, has been almost universal; for, although many have pretended to an equal temperament, yet the methods which they have employed to attain it have been evidently defective. It appears to me, that every purpose may be answered, by making C : E too sharp by a quarter of a comma, which will not offend the nicest ear; E : G♯, and A♭ : C,

equal; F♯ : A♯ too sharp by a comma; and the major thirds of all the intermediate keys more or less perfect, as they approach more or less to C in the order of modulation. The fifths are perfect enough in every system. The results of this method are shown in Table XII. In practice, nearly the same effect may be very simply produced, by tuning from C to F, B♭, E♭, G♯, C♯, F♯, six perfect fourths; and C, G, D, A, E, B, F♯, six equally imperfect fifths, Fig. 112. If the unavoidable imperfections of the fourths be such as to incline them to sharpness, the temperament will approach more nearly to equality, which is preferable to an inaccuracy on the other side. An easy method of comparing different systems of temperament is exhibited in Fig. 113, which may easily be extended to all the systems that have ever been invented.

Table XII.

A		B		C	
C	50000	1 C	+ .0013487	1 A, E	− .0023603
B	53224	2 G, F	.0019006	2 D, B	.0029192
B♭	56131	3 D, B♭	.0024525	3 G, F♯	.0034641
A	59676	4 A, E♭	.0034641	4 C, C♯	.0044756
G♯	63148	5 E, A♭	.0044756	5 F, G♯	.0049353
G	66822	6 B, C♯	.0049353	6 ♭, E♭	.0053950
F♯	71041	7 F♯	.0053950		
F	74921				
E	79752				
E♭	84197				
D	89304				
C♯	94723				
C	100000				

D	
1 E♭, G♯, C♯, F♯	− .0000000
2 F, B♭, E, B	.0004597
3 C, G, D, A	.0010116

A shows the division of a monochord corresponding to each note in the system proposed. B, the logarithm of the temperament of each of the major thirds. C, of the minor thirds. D, of the fifths; C and D being both negative.

Thus, Sir, I have endeavoured to advance a few steps only, in the investigation of some very obscure but interesting subjects. As far as I know, most of these observations are new; but, if they should be found to have been already made by any other person, their repetition in a connected chain of inference may still be excusable. I am persuaded also, that at least some of the positions maintained are incontrovertibly consistent with truth and nature; but, should further experiments tend to confute any opinions that I have suggested, I shall relinquish them with as much readiness as I have long since abandoned the hypothesis which I once took the liberty of submitting to the Royal Society, on the functions of the crystalline lens.*

I am, &c.

THOMAS YOUNG.

Emanuel College, Cambridge,
8th July, 1799.

Explanation of the Figures.

Figs. 61—66. The section of a stream of air from a tube of .07 inch in diameter, as ascertained by measuring the breadth of the impression on the surface of a liquid. The pressure impelling the current, was in Fig. 61, 1 inch. Fig. 62, 2. Fig. 63, 3. Fig. 64, 4. Fig. 65, 7. Fig. 66, 10.

Figs. 67—72. A similar section, where the tube was .1 in diameter, compared with the section as inferred from the experiments with two gauges, which is represented by a dotted line. From this comparison it appears, that where the velocity of the current was small, its central parts only displaced the liquid; and that, where it was great, it displaced, on meeting with resistance, a surface somewhat greater than its own section. The pressure was in Fig. 67, 1. Fig. 68, 2. Fig. 69, 3. Fig. 70, 4. Fig. 71, 7. Fig. 72, 10.

Figs. 73—80. A, the half section of a stream of air from a tube .1 in diameter, as inferred from experiments with two water gauges. The pressure was in Fig. 73, .1. Fig. 74, .2. Fig. 75, .5. Fig. 76, 1. Fig. 77, 3. Fig. 78, 5. Fig. 79, 7. Fig. 80, 10. The fine lines, marked B show the result of the observations with an aperture .15 in diameter opposed to the stream; C with .3; and D with .5.

Figs. 81—83. A, the half section of a current from a tube .3 in diameter, with a pressure of .5 of 1, and of 3. B shows the course of a portion next the axis of the current, equal in diameter to those represented by the last figures.

* Supra, p. 12.

Fig. 61.

Fig. 62.

Fig. 63.

Fig. 64.

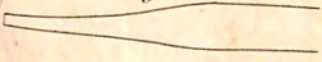
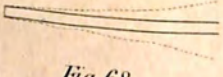


Fig. 65.

Fig. 66.

Fig. 67.



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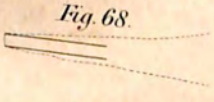

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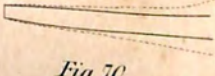

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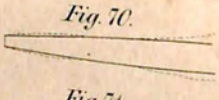

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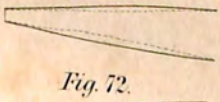

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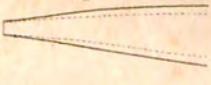

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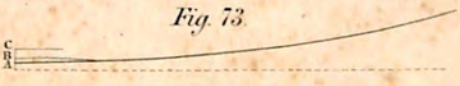

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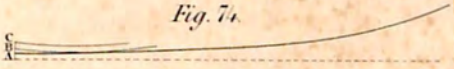

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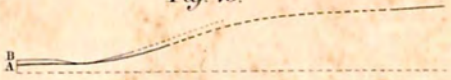

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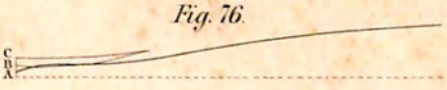

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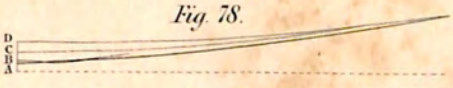

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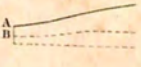

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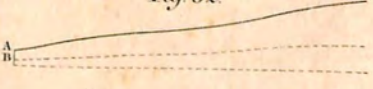

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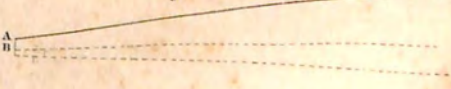

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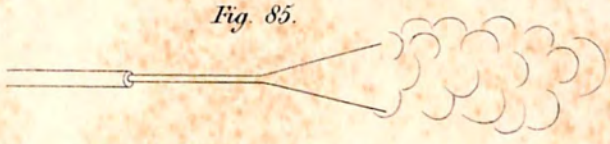

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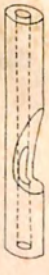

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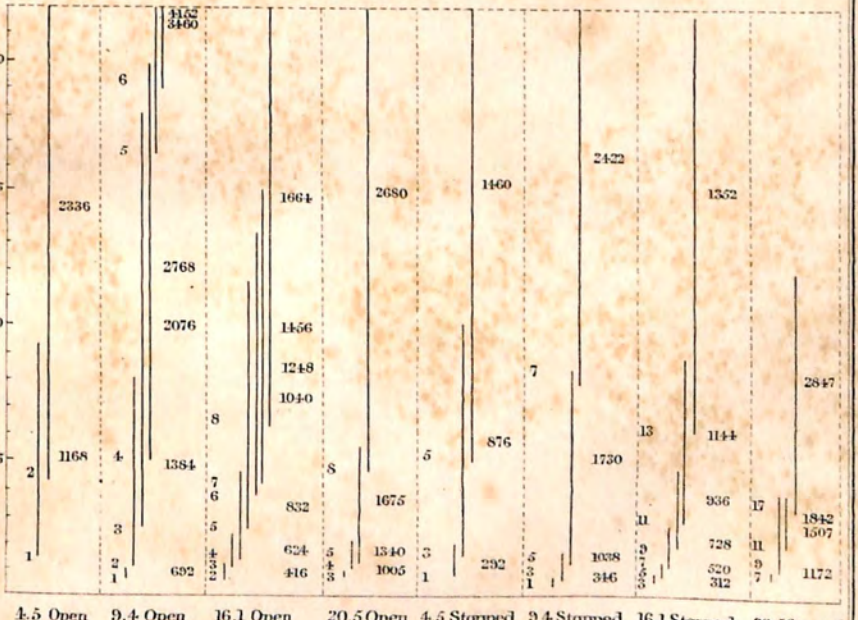

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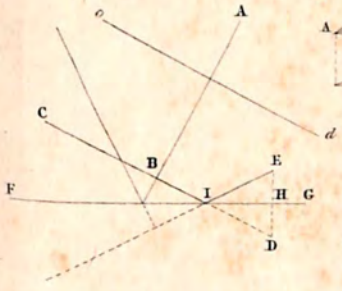

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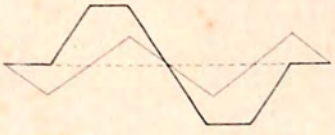

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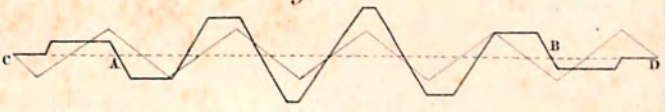

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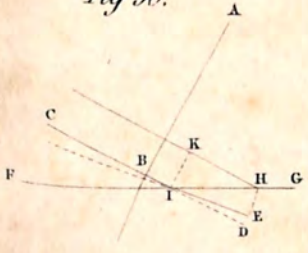

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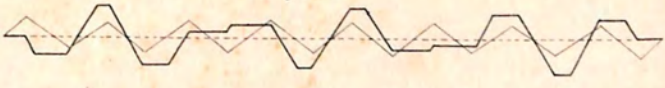

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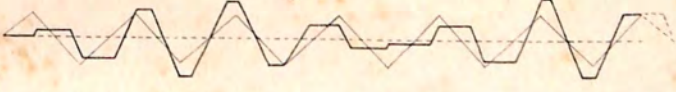

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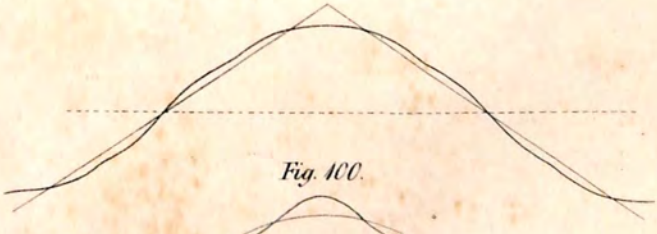

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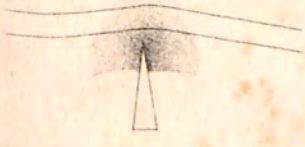

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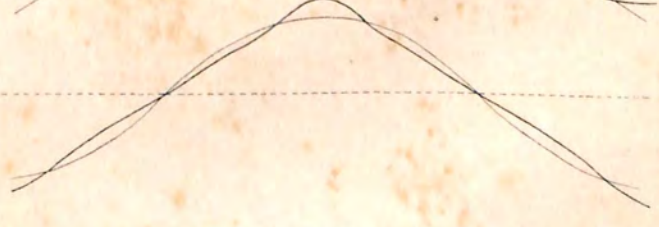

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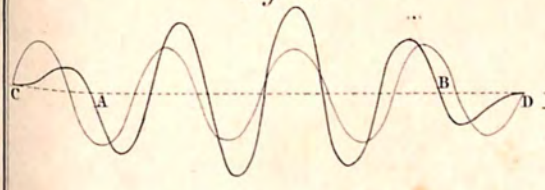

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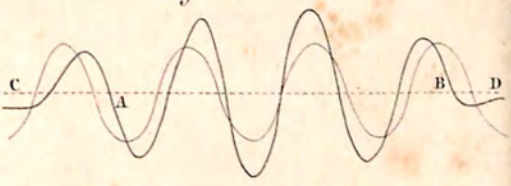

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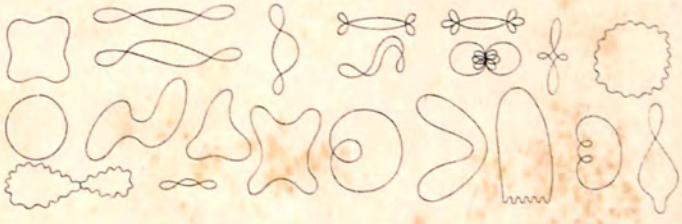

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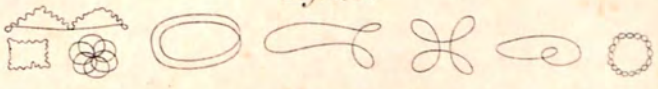

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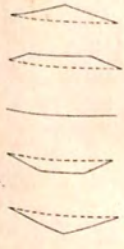

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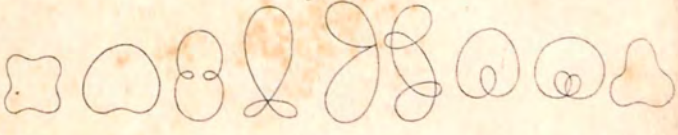

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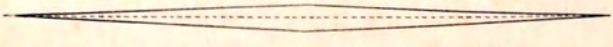

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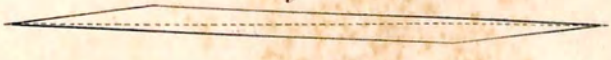

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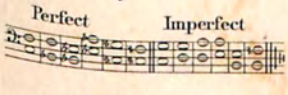

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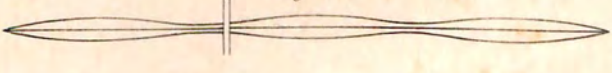

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Fig. 113.

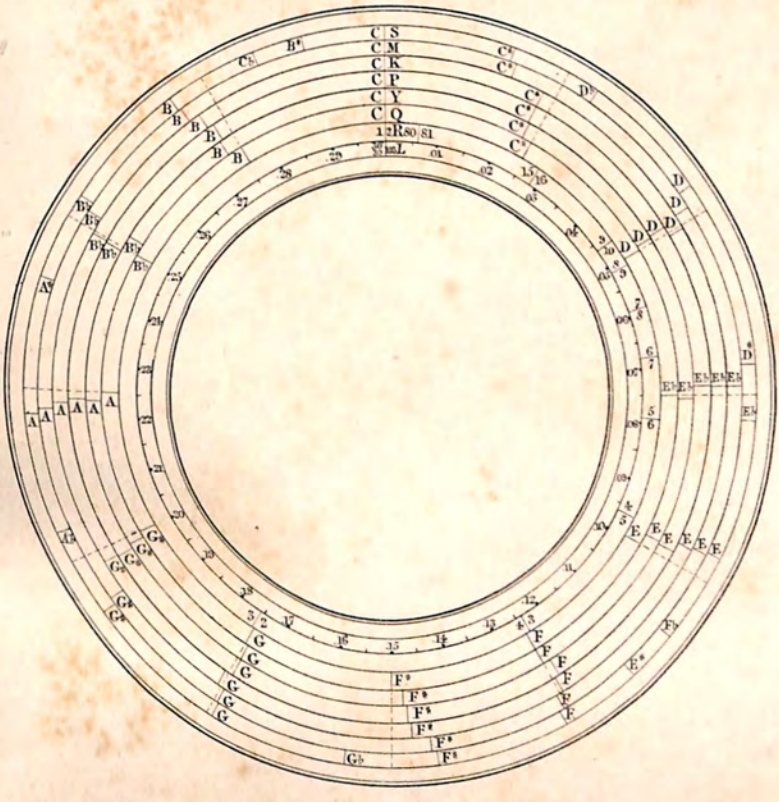

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Fig. 84. The appearance of a stream of smoke forced very gently from a fine tube. Figs. 85 and 86, the same appearance when the pressure is gradually increased.

Fig. 87. See Section III.

Fig. 88. The perpendicular lines over each division of the horizontal line show, by their length and distance from that line, the extent of pressure capable of producing, from the respective pipes, the harmonic notes indicated by the figures placed opposite the beginning of each, according to the scale of 22 inches parallel to them. The larger numbers, opposite the middle of each of these lines, show the number of vibrations of the corresponding sound in a second.

Figs. 89—93. See Section X.

Fig. 94. The combination of two equal sounds constituting the interval of an octave, supposing the progress and regress of the particles of air equable. Figs. 95, 96, 97, a similar representation of a major third, major tone, and minor sixth.

Fig. 98. A fourth, tempered about two commas.

Fig. 99. A vibration of a similar nature, combined with subordinate vibrations of the same kind in the ratios of 3, 5, and 7.

Fig. 100. A vibration represented by a curve of which the ordinates are the sines of circular arcs increasing uniformly, corresponding with the motion of a cycloidal pendulum, combined with similar subordinate vibrations in the ratios of 3, 5, and 7.

Figs. 101 and 102. Two different positions of a major third, composed of similar vibrations, as represented by figures of sines.

Fig. 103. A contracted representation of a series of vibrations. A, a simple uniform sound. B, the beating of two equal sounds nearly in unison, as derived from rectilinear figures. C, the beats of two equal sounds, derived from figures of sines. D, a musical consonance making by its frequent beats a fundamental harmonic. E, the imperfect beats of two unequal sounds.

Fig. 104. Various forms of the orbit of a musical chord, when inflected, and when struck.

Fig. 105. Forms of the orbit, when the sound is produced by means of a bow.

Fig. 106. Epitrochoidal curves, formed by combining a simple rotation or vibration with other subordinate rotations or vibrations.

Figs. 107 and 108. The successive forms of a tended chord, when inflected and let go, according to the construction of De la Grange and Euler.

Fig. 109. The appearance of a vibrating chord which had been inflected in the middle, the strongest lines representing the most luminous parts.

Fig. 110. The appearance of a vibrating chord, when inflected at any other point than the middle.

Fig. 111. The appearance of a chord, when put in motion by a bow applied nearly at one third of the length from its end.

Fig. 112. The method of tuning recommended for common use.

Fig. 113. A comparative view of different systems of temperament. The whole circumference represents an octave. The inner circle L is divided into 30103 parts, corresponding with the logarithmical parts of an octave. The next circle R shows the magnitude of the simplest musical and other ratios. Q is divided into twelve equal parts, representing the semitones of the equal temperament described by Zarlino, differing but little from the system of Aristoxenus, and warmly recommended by Marpurg and other late writers. Y exhibits the system proposed in this paper as the most desirable; and P the practical method nearly approaching to it, which corresponds with the eleventh method in Marpurg's enumeration, except that, by beginning with C instead of B, the practical effect of the temperament is precisely inverted. K is the system of Kirnberger and Sulzer; which is derived from one perfect third, ten perfect and two equally imperfect fifths. M is the system of mean tones, the *sistema participato* of the old Italian writers, still frequently used in tuning organs, approved also by Dr. Smith for common use. S shows the result of all the calculations in Dr. Smith's harmonics, the system proposed for his changeable harpsichord, but neither in that nor any other form capable of practical application.

No. IV.

AN ESSAY ON

CYCLOIDAL CURVES.

Felices animæ, quibus hæc cognoscere primis,
Inque domos superum scandere, cura fuit.

On Mathematical Symbols.

Many of the most celebrated modern mathematicians have been disposed to pride themselves on the very great superiority which they attribute to the modern methods of calculation over those which were known to the ancients. That, in the course of so many centuries, mathematical sciences, like all others, should have been very considerably advanced, is no more than must have been expected, from the great number of persons who have employed their talents in the cultivation of those sciences. But if we examine the matter impartially, we shall have reason to

* This Essay was the third of a series which appeared in the British Magazine for 1800, under the title of "The Leptologist," on the subjects indicated in the following motto:

"Grammaticus, rhetor, geometres, pictor, aliptes,
Augur, schœnobates, medicus, magus, omnia novit
Græculus esuriens."

The introductory observations afford a good insight into his peculiar views of the superiority of geometrical to analytical processes of investigation, on the grounds of their greater simplicity, clearness, and brevity. There is manifested in the criticisms which are intermixed with them a certain flippancy and bitterness of expression, which too frequently appeared in his replies, at least in early life, to those who controverted his opinions: the character of the introductory remarks upon some propositions contained in a paper by Lord Brougham, printed in the Philosophical Transactions for 1798, was probably not without its influence in provoking those persevering attacks upon his optical researches, which appeared in some of the earlier Numbers of the Edinburgh Review.

The investigations contained in this Essay, though neither very important nor original, are characteristic of Dr. Young's remarkable aptitude for applying a somewhat impure geometry to the solution of problems, to which an approach is usually required to be made by processes of a much more regular and elaborate character.—*Note by the Editor.*

believe not only that mathematics have been as slow in their real advancement as any other part of philosophy, but that the moderns have very frequently neglected the more essential, for frivolous and superficial advantages. To say nothing of the needless incumbrances of new methods of variations, of combinatorial analyses, and of many other similar innovations, the strong inclination which has been shown, especially on the Continent, to prefer the algebraical to the geometrical form of representation, is a sufficient proof, that, instead of endeavouring to strengthen and enlighten the reasoning faculties, by accustoming them to such a consecutive train of argument as can be fully conceived by the mind, and represented with all its links by the recollection, they have only been desirous of sparing themselves as much as possible the pains of thought and labour by a kind of mechanical abridgment, which, at best, only serves the office of a book of tables in facilitating computations, but which very often fails even of this end, and is at the same time the most circuitous and the least intelligible. These philosophers are like the young Englishman on his travels, who visits a country by driving with all possible speed from place to place by night, and refreshing his fatigues in the daytime by lounging half asleep at his hotel. Undoubtedly there are some countries through which one may reasonably wish to travel by night; and undoubtedly there are some cases where algebraical symbols are more convenient than geometrical ones: but when we see an author exerting all his ingenuity in order to avoid every idea that has the least tincture of geometry, when he obliges us to toil through immense volumes filled with all manner of literal characters, without a single diagram to diversify the prospect, we may observe with the less surprise that such an author appears to be confused in his conception of the most elementary doctrines, and that he fancies he has made an improvement of consequence, when, in fact, he is only viewing an old object in a new disguise. It happens frequently in the description of curves, and in the solution of problems, that the geometrical construction is very simple and easy, while it almost exceeds the powers of calculus to express the curve or the locus of the equation in a manner strictly algebraical; and,

indeed, the astonishing advances that were made in a comparatively short time by Euclid, by Apollonius, and, above all, by Archimedes, are sufficient to prove that the method of representation which they employed could not be very limited in its application; and the precision and elegance with which the method of geometrical fluxions is treated by Newton and Maclaurin form a strong contrast to the tedious affectation of abstraction and obscurity which unfortunately pervades the writings of many great mathematicians of a later date. It would be of inestimable advantage to the progress of all the sciences if some diligent and judicious collector would undertake to compile a complete system of mathematics; not as an elementary treatise, nor as a mere index of reference, but to contain every proposition, with a concise demonstration, that has ever yet been communicated to the public. Until this is done nothing is left but for every individual who is curious in the search of geometrical knowledge to look over all the mathematical authors and all the literary memoirs of the last and present centuries: for without this he may very easily fancy he has made discoveries, when the same facts had been known and forgotten long before he existed. An instance of this has lately occurred to a young gentleman in Edinburgh, a man who certainly promises, in the course of time, to add considerably to our knowledge of the laws of nature. The tractory, tractrix, or equitangential curve, was first described by Huygens, and afterwards more fully by M. Bomie, Mém. Acad. 1712, and by Mr. Perks, Ph. Tr. v. xix. n. 345, Abr. v. iv. p. 456. Bomie and Perks have shown many remarkable properties belonging to it; and one in particular, which may be briefly demonstrated, that it is the involute of the catenaria: for since the equation of the catenaria is $zz = 2a\dot{x} + x\dot{x}$, we have $z\dot{z} = a\dot{x} + x\dot{x}$, and $\dot{x} : \dot{z} :: z : a+x$, therefore the vertex of the right-angled triangle, of which the base is the evolved radius, and the hypotenuse a line parallel to the axis of the curve, describes a right line; and the perpendicular of this triangle is always $= a$, and is the constant tangent of the curve described by the evolution. Cotes has also, in his Logometria, investigated the properties of the tractrix of the circle. Bernoulli observed, in

52063

1730, that the tractrix was one of the tautochronous curves in a resisting medium. In 1736 it was the subject of a dispute between MM. Clairaut and Fontaine: it is not yet entirely forgotten on that spot of academic ground which gave birth to the discoveries of Newton; and its equation is to be found in a work no less common than Emerson's Fluxions, nearly in the same form as that which is published as new in the Philosophical Transactions for 1798. We find in the same paper a new method of dividing an elliptic area in a given ratio; but the curve which the author calls a cycloid is the companion of a trochoid, and is only a distortion of the figure by which Newton had very simply and elegantly solved the same problem. It is unnecessary to compare the attempt to demonstrate the incommensurability of an oval with the Newtonian method: since Dr. Waring's proof, deduced from the nature of the equation of limits, is decidedly more satisfactory than any other hitherto made known. On the whole it appears that this ingenious gentleman has been somewhat unfortunate in the choice of those problems which he has selected as specimens of the elegance of the modern mode of demonstration; whether those which he has brought forward without proof would have furnished him with a more favourable opportunity for the display of neatness and accuracy, may be more easily determined whenever he may think proper to lay before the public their analysis, construction, and demonstration, at full length. But, allowing the superiority of the modern calculations in many cases, their great advantage appears to be derived from the methods of series and approximations; indeed, however we may wish to adhere to the rigour of the ancient demonstrations, it is absolutely necessary, for the purposes of the higher geometry, to extend, in some measure, the foundations which the ancients laid in their postulates. Perhaps the most material addition may be comprehended in this form: "Let it be granted that any curve line may be drawn whenever an indefinitely great number of points may be geometrically found in, or indefinitely near to, that line." No doubt it is mathematically impossible to comply with this postulate; but it must be remembered that it is also impossible to draw, with strictly mathematical accuracy, a right line or a

circle; but in both cases we can approach sufficiently near to the truth for practice: and it appears to be more convenient to consider such curves as are thus described as belonging to geometry, than to limit the number of geometrical curves, according to Descartes, to those of which the ordinate and abscis are comparable by an algebraical equation. This postulate forms the connecting link between rational and irrational quantities, between the infinite and the indefinite, between perfect resemblance and identity; and the irrational geometry which has long been tacitly built on it, exhibits the principal advantages of analytical calculus in a more elegant form. The ground-work of this irrational geometry is found in the method of exhaustions of Euclid and Archimedes; and it has been employed more or less generally by Descartes, Newton, Cotes, Roberval, Varignon, Delahire, Maclaurin, and many other mathematicians. In the annexed essay on cycloidal curves the geometrical form of fluxions, or, more properly speaking, the Newtonian method of ultimate ratios, has principally been adopted; and it is presumed that by a comparison with algebraical calculations on the same subjects, the superior perspicuity and conciseness of this method will readily appear.

On Cycloidal Curves.

"Il semble qu'une destinée particulière attachée à la cycloïde lui donne préférablement aux autres courbes un plus grand nombre de propriétés remarquables."

Definition I.

When a circle is made to rotate on a rectilinear basis, the figure described on the plane of the basis by any point in the plane of the circle, is called a trochoid. A circle concentric with the generating circle, and passing through the describing point, may be called the describing circle.

Definition II.

If the describing point is in the circumference of the rotating

circle, the two circles coincide, and the curve is called a cycloid.

Definition III.

If a circular basis be substituted for a rectilinear one, the trochoid will become an epitrochoid, and the cycloid an epicycloid.

Scholium 1. These terms have hitherto been too promiscuously employed; the terms cycloid and trochoid have been used indifferently; and the term epicycloid has comprehended the epitrochoid, the terms prolate and contracted being sometimes added to imply that the describing point is within or without the generating circle. The interior epicycloid and epitrochoid may very properly be distinguished by the names hypocycloid and hypotrochoid, whenever they are the separate objects of consideration. The different species of epicycloids may be denominated according to the number of their cusps, combined with that of the entire revolutions which they comprehend; for instance, the epicycloid described by a circle on an equal basis is a simple unicuspidate epicycloid; and if the diameter of the generating circle be to that of the basis as 5 to 2, the figure will be a quintuple bicuspidate epicycloid. If the describing circle of a trochoid or cycloid be so placed as to touch the middle of the basis, and each of the ordinates parallel to the basis be diminished by the corresponding ordinate of the circle, the curve thus generated has been denominated the companion of the trochoid or cycloid, the figure of sines, and the harmonic curve.

Scholium 2. The invention of the cycloid has been attributed by Wallis, Ph. Tr. for 1697, n. 229, to Cardinal Cusanus, who wrote about the year 1450: but it seems to be at least as probable that the curve which appears in Cusanus's figure was meant for the semicircle employed in finding a mean proportional. Bovillus, in 1501, has a juster claim to the merit of the invention of the cycloid and trochoid, if it can be any merit to have merely imagined such curves to exist. In 1599 Galileo gave a name to the common cycloid, and attempted its quadrature; but having been accidentally misled by repeated experi-

ments on the weight of a flat substance cut into a cycloidal form, he fancied that the area bore an incommensurable ratio to that of the circle, and desisted from the investigation. Mersennus described the cycloid, in 1615, under the name of la trochoïde, or la roulette, but he went no further. Roberval seems to have first discovered the comparative quadrature and rectification of the cycloid, and the content of a cycloidal solid, about the year 1635, but his treatise was not printed until 1695. Torricelli, in 1644, first published the quadrature and the method of drawing a tangent. Wallis gave, in 1670, a perfect quadrature of a portion of the cycloid. The epicycloid is said to have been invented by Rœmer; its rectification and evolute were investigated by Newton in the Principia, published in 1687. In 1695 Mr. Caswell showed the perfect quadrability of a portion of the epicycloid, and Dr. Halley immediately published an extension of Caswell's discovery, together with a comparison of all epitrochoidal with circular areas. M. Varignon is also said to have reduced the rectification of the epitrochoid to that of the ellipsis, in the same year. Nicole, Delahire, Pascal, Réaumur, Maclaurin, the Bernoullis, the commentators on Newton, and many others, have contributed to the examination of cycloidal curves, both in planes and in curved surfaces; and Waring, the most profound of modern algebraists, has considerably extended his researches upon the nature of those lines which are generated by a rotatory progression of other curves. In the present essay the most remarkable properties of cycloidal curves are deduced in a simpler and more general manner than appears to have been hitherto done, the equations of several species are investigated, a singular property of the quadricuspidate hypocycloid is demonstrated, and the peculiarities of the spiral of Archimedes are derived from its generation as an epitrochoid.

PROPOSITION I.—*Theorem.* (Fig. 114.)

In any curve generated by the rotation of another on any basis, the right line joining the describing point, and the point of contact of the generating curve and the basis, is always perpendicular to the curve described.

It may by some be deemed sufficient to consider the generating curve as a rectilinear polygon of an infinite number of sides; since, in this point of view, the proposition requires no further demonstration; and, indeed, Newton and others have not scrupled to take it for granted: but it is presumed that a more rigid proof will not be considered as superfluous. Let M be the describing point, and P the point of contact; and let LO, MP, and NQ, be successive positions of the same chord of the generating curve at infinitely small distances; then it is obvious, and easily demonstrable, that the arcs OP and PQ, described by the point P of the generating curve in its passage from O to P, and from P to Q, will be perpendicular to the basis at P, and will, therefore, touch each other. Let the arcs L, IMK, and N, be described with the radius PM, on the centres O, P, and Q. Then the curve described by M will touch IMK; for since O and Q lie ultimately in the same direction from P, if L be above IMK, N will also be above it, since these points must be in the circles L and N, and infinitely near to M; and if L is below IMK, N, for the same reason, must be below it; and M is common to the circle and the curve, therefore the curve touches the circle IMK at M, and is perpendicular to the radius PM.

Proposition II.—*Problem.*

To draw a tangent to a cycloidal curve at any given point.

On the given point as a centre describe a circle equal to the describing circle of the curve; and from the intersection of this circle, with the line described by the centre of the generating circle, let fall a perpendicular on the basis; the point thus found will be the point of contact, and the tangent will be perpendicular to the right line joining this point of contact and the given point, by the first proposition. It will be obvious, from inspection, which of the two intersections of the circle to be described, with the track of the centre, is to be taken as the place of that centre corresponding to the given point.

PROPOSITION III.—*Problem.* (Fig. 115.)

To find the length of an epitrochoid.

Let C be the centre of the basis VP, K that of the rotating circle PR, and of the describing circle GL, P the point of contact, and M the describing point. Then joining MXC, and supposing VX to be an element representing the motion of the point P in either the basis or the generating circle, draw the arc MN on the centre C, and join CVN, then NM will represent the motion of the point M as far as it is produced by the revolution round the centre C: take MO to VX as GK to PK, then MO will be the motion of M arising from the revolution round K, and NO will be the element of the curve produced by the joint motion. Let CH be parallel to PM, then CX or CP : CM : : VX : MN, and PK : MK : : CP : HM : : VX : MO, therefore CM : HM : : MN : MO, and these lines being perpendicular to CM, HM, the triangle NMO is similar to CMH, and MN : NO : : CM : CH, hence, CP : CH : : VX : NO. Take PY to CP as PK to CK, then CH : CP : : PM : PY : : NO : VX. On L describe the circle PFB, and draw IMLF: let FD be perpendicular to PRB, take DE to DF as PG to PL, and E will be always in the ellipsis BEP: let AE and AF be tangents to the ellipsis and circle at E and F; then the increment of the arc BF will be to MO as PL to GL, and to VX as PL to PR. Join GM, and parallel to it draw PI; then PIL is a right angle, and ILP = AED, and IM : IL : : PG : PL : : DE : DF, by construction; therefore the figure IPML is similar to DAEF, as PL to PM so is AF to AE, and so is the increment of the arc BF to that of BE; but the increment of BF is to VX as PL to PR, therefore the increment of BE is to VX as PM to PR. Now, it was proved that NO : VX : : PM : PY; therefore the increment of BE is to NO as PY to PR, or as CP to 2CK; and the whole elliptic arc BE is to the whole SM as the radius of the basis to twice the distance of the centres.

Corollary 1. The fluxion of every cycloidal arc is pro-

portional to the distance of the describing point from the point of contact.

Corollary 2. In the epicycloid the ellipsis coincides with its axis BP, and the arc BE with BD, which is double the versed sine of half the arc GM, in the describing or generating circle: therefore the length of the curve is to this versed sine as four times the distance of the centres to the radius of the basis.

PROPOSITION IV.—*Problem.* (Figs. 115, 116.)

To find the centre of curvature of an epitrochoid.

Let PY be, as in the last Proposition, to CP as PK to CK, and on the diameter PY describe the circle PZY, cutting PO in Z: take OW a third proportional to OZ and OP, and W will be the centre of curvature. For, let QP = VX be the space described by P, while NO is described by O; it is obvious from Prop. I. that the intersection of NQ and OP must be the centre of curvature. Let QΓ be perpendicular to PO, and ΓΔ parallel to QN; then, by Prop. III. NO : VX or QP :: PO : PY, but by similar triangles QP : QΓ :: PY : PZ; therefore NO : QΓ :: PO : PZ, and by division, NO : ΔO :: OP : OZ, and by similar triangles :: OW : OΓ or OP.

Corollary. When Z coincides with O or M, OW is infinite; therefore whenever PZY intersects the describing circle, the epitrochoid will have a point of contrary flexure at the same distance from C as this intersection: and the circle PZY is given when the basis and generating circle are given, whatever the magnitude of the describing circle may be.

PROPOSITION V.—*Problem.* (Fig. 117.)

To find the evolute of an epicycloid.

In the epicycloid SM, the point M being in the circumference of PMR, PZ will be to PM in the constant ratio of PY to PR, and MZ to PM as RY to PR, and PM to MW in the same ratio; hence PM : PW :: RY : PY :: CR : CP, therefore the point W is always in a circle PWΞ of which the

radius is to PK in that proportion, and which touches SP in P. On the centre C describe a circle ΛΞΘ touching PWΞ in Ξ; then, since CR : CP : : PR : PΞ, we have by division CR : CP : : CP : P, and the circle PWΞ being to ΛΞΘ as PMR to SP, the arc PM being equal to SP, the similar arc PW will be equal to ΛΞ, and, taking ΛΞΘ = PWΞ, ΞΘ will be always equal to ΞW, and W in a curve ΘWS similar to SM, of which it is the evolute.

PROPOSITION VI.—*Problem.* (Fig. 118.)

To find the area of an epitrochoid.

On the centre C describe a circle touching the epitrochoid in S, take GΠ to GC as PR to PC, and let the circle GΦΠ describe on the basis SG the epicycloid SΦ. Then taking GM always to GΦ as GL to GΠ, M will be in the epitrochoid SM; for the angular motion of the chord GΦ, is the same as that of GM in the primary epitrochoid. Let SΩ be the evolute of SΦ, and GWΞ its generating circle. On diameters equal to ΞG, ΞL, and ΞΠ, describe three circles, AD, AE, and AF, touching the right line AB in A; let the angle BAD be always equal to GΠΦ, and it is evident that AD, AE, and AF, will be equal respectively to WG, WM, and WΦ. But the angular motion of WG on W being equal to the sum of the angular motions of GM on G and CG on C, is to that of AF, or of GM, or half that of KM, in the ratio of CΠ to CG, or CR to CP; therefore the fluxions of the areas SWG, SWM, and SWΦ are to those of the segments AD, AE, and AF, in the same ratio; and that ratio being constant, the whole areas, and their differences, are also respectively to each other as CR to CP.

Scholium. The quadrable spaces of Halley are those which are comprehended between the arc of the epitrochoid, that of the describing circle, and that of a circle concentric with the basis and cutting the describing circle at the extremities of its diameter.

PROPOSITION VII.—*Problem.* (Fig. 119.)

To find a central equation for the epicycloid.

Let CT be perpendicular to RT, the tangent at the point M, then PMR will be a right angle, and PM parallel to CT. On the centre C describe through M the circle MNO, and let MQ be perpendicular to RO. Then the rectangle OQN = PQR, OQ : PQ :: QR : QN, by addition OQ : PQ :: OR : PN; hence by division OP : PQ :: IR : PN, and $PQ = \frac{OPN \text{ or } INP}{IR}$. But $PMq = PR \times PQ = \frac{PR}{IR} \times INP$: and by similar triangles CT : CR :: PM : PR, whence $CTq = \frac{CRq}{PRq} \times PMq = CRq \times \frac{INP}{IRP}$. Let MZ and RY be tangents to SP, then $INP = MZq$, and $IRP = RYq$, $CT = CR \times \frac{MZ}{RY}$, and CT will be to MZ in the constant ratio of CR to RY. Putting CP = a, CR = b, CM = s, CT = u, then $uu = bb\,\frac{ss-aa}{bb-aa}$.

PROPOSITION VIII.—*Problem.* (Fig. 120.)

To find a geometrical equation for the conchoidal epitrochoid.

Let CP = PK. On the centre C describe a circle equal to GM, cutting SC in Z. Join MZF, then the arc DZ = GM, and MZ is parallel to CK, therefore EF is also equal to DZ or GM, CF is parallel to KM, and MF = CK : therefore this epitrochoid is the curve named by Delahire the conchoid of a circular basis, as was first observed by Réaumur in 1708, and afterwards by Maclaurin in 1720. Call CK, a, DE, b, ZH, x, HM, y, ZM, s; and let ZI be perpendicular to CK; then $FZ = a-s$, $CI = \frac{a-s}{2}$, and, CIZ and ZHM being similar, CZ : CI :: ZM : ZH, or $\frac{b}{2} : \frac{a-s}{2} :: s : x$; hence $bx = as-ss$ $bx + ss = as$, $b^2x^2 + 2bxs^2 + s^4 = a^2s^2$, and by substituting for s^2, $x^4 + 2bx^3 - a^2x^2 + b^2x^2 + 2x^2y^2 + 2bxy^2 + y^4 - a^2y^2 = 0$.

Corollary 1. Join FN, and complete the parallelogram MFNL, then since EF = DZ = EN, FN is perpendicular to

EK, and ML to NL, and, NL being always equal to FM or CK, L is always in a circle described on the centre N, LM a tangent to that circle, and ZM a perpendicular to that tangent drawn from the point Z.

Corollary 2. Fig. 121. The unicuspidate epicycloid admits of a peculiar central-equation, with respect to the point S. Call SM, s, and let ST $= u$ be perpendicular to the tangent MT, then $u = \left|\frac{s^3}{2a}\right|^{\frac{1}{2}}$. For the triangles SIP and MTS being similar, and IP being half of SM, or s, SP $= \sqrt{\frac{as}{2}}$, SPq : SMq :: IPq : STq or $\frac{as}{2} : s^2 :: \frac{ss}{4} : u^2$, and $2au^2 = s^3$.

Corollary 3. Fig. 121. The unicuspidate epicycloid is one of the caustics of a circle. For making the angle CRY = MRC $= \frac{1}{2}$ MKP $= \frac{1}{2}$ SCP, the triangle CRY is isosceles, and CY is constant; so that all rays in the direction of the tangent MR will be reflected by the circle QR towards Y, and consequently SM will be the caustic of a radiant point at Y.

PROPOSITION IX.—*Problem.* (Fig. 122.)

To find a geometrical equation for the tricuspidate hypocycloid.

Let PA and MF be perpendicular to CS. Join PMB, KM, RMG, and PD. Then the angle APB is equal to the difference of APC and MPR, or to that of their complements PRM, PCA: but PRM $= \frac{1}{2}$ PKM $= \frac{1}{3} -$ PCA, therefore APB $= \frac{1}{2}$ PCA $=$ ADP $=$ APS, and the triangles APS and APB are similar and equal. Let SC $= a$, SF $= x$, FM $= y$, and SB $= r$. Then SA : SP :: SP : SD, and SP $= \sqrt{ar}$. Draw PE perpendicular to BP; then BE $=$ SD $= 2a$, BC $= a - r$, EC $= 3a - r$, and by similar triangles, CP : CR :: EC : CG $= \frac{1}{3}$ EC $= a - \frac{1}{3}r$; therefore GB $= \frac{2}{3}r$; but BE : BG :: BP : BM, or $2a : \frac{2}{3}r$: : $\sqrt{ar} : \frac{r}{3a}\sqrt{ar} =$ BM; again BP : BM :: BA : BF, or $\sqrt{ar} : \frac{r}{3a}\sqrt{ar} :: \frac{r}{2} : \frac{rr}{6a}$, and SF $= x = r - \frac{rr}{6a}$, $6ax = 6ar - rr$, and $r = 3a \pm \sqrt{9aa - 6ax}$. But MF$q$ $=$ BM$q -$ BFq, or $y^2 = \frac{r^3}{9a} - \frac{r^4}{36aa}$, and

$36a^2y^2 = 4ar^3 - r^4$. By adding to this the square of the former equation, and proceeding in the same manner to exterminate r, we obtain an equation of the value of x and y, which, when the surds are brought to the same side, and the square of the whole is taken, is at last reduced to $x^4 - \frac{4}{3}ax^3 + 2x^2y^2 - 12axy^2 + y^4 + 12a^2y^2 = 0$, a regular equation of the fourth order.

Scholium. The equation of the corresponding hypotrochoids may be investigated nearly in the same manner, by dividing PR and PM in a given ratio, but the process will be somewhat more tedious.

PROPOSITION X.—*Problem.* (Fig. 123.)

To find a geometrical equation for the bicuspidate epicycloid.

Let CP = PR. Join RMT, PM, PD; draw CT perpendicular to RT, TE to CR, and EG, MB, RA, to SC. Then the angle DRP = $\frac{1}{2}$ MKP = SCP, and by equal triangles, RA = CT, and RD = CD, and by similar triangles RM : RP :: RE : RT, and RP : RD :: RT : RC; therefore RM : RD :: RE : RC, and ME is parallel to SC, and EG = BM. Put CP a, BC = x, BM = y, CM = s, CT = u; then by Prop. VII. $u^2 = \frac{4}{3}\overline{s^2 - a^2}$; or $\frac{3}{4}u^2 = s^2 - a^2$; but RC : CT :: CT : CE :: CE : EG, or y, hence $y = \frac{u^3}{4aa}$ $u^6 = 16a^4y^2$ $\frac{27}{64}u^6 = \frac{27}{4}a^4y^2 = \overline{ss - aa}^3 = \overline{xx + yy - aa}^3$; whence by involution the equation of the sixth order may be had at length.

Corollary. Since CRM = SCR, a ray in the direction of the tangent MR will be reflected by a circle FR always parallel to SC: therefore SM is the caustic of the circle FR when the incident rays are parallel to CS.

PROPOSITION XI.—*Problem.* (Fig. 124.)

To find a geometrical equation for the quadricuspidate hypocycloid.

Let CR = PR, then the angle PRM = $\frac{1}{2}$PKM = 2PCS, RAC ACR, RA = RC = RB = RP, AB = SC, and drawing the perpendiculars CT, TD, TE, and MF, RM = RT, AM = BT, AF = EC, FC = AE, and FM = BD. Let SC = a, FC = x, FM

$=y$, CM $=s$, CT $=u$; then AB : AC :: AC : AT :: AT : AE, whence AT $= \overline{axx}^{\frac{1}{3}}$, and in the same manner BT $= \overline{ayy}^{\frac{1}{3}}$; and CT being a mean proportional between AT and TB, $u^2 = \overline{a^2x^2y^2}^{\frac{1}{3}}$, and $u^6 = a^2x^2y^2$. But by Prop. VII., $3u^2 = a^2 - s^2$, therefore $27a^2x^2y^2 = \overline{a^2 - s^2}^3 = \overline{a^2 - x^2 - y^2}^3$; whence the equation may be had at length by involution. The same result may be obtained by Dr. Waring's method of reduction, from $\overline{axx}^{\frac{1}{4}} + \overline{ayy}^{\frac{1}{3}} = a$.

Corollary. Since the portion of the tangent AB intercepted between the perpendiculars AC, BC, is a constant quantity, this hypocycloid may in that sense be called an equitangential curve; and the rectangular corner of a passage must be rounded off into the form of this curve in order to admit a beam of a given length to be carried round it.

Proposition XII.—*Problem.*

To investigate those cases in which the general propositions either fail or require peculiar modifications.

Case 1. Fig. 125. If the generating circle be considered as infinitely small, or the basis as infinitely large, so as to become a straight line, the epicycloid will become a common cycloid, and the ratio of CP to CK in Prop. III. cor. 2, becoming that of equality, the length of the arc SM will be four times the versed sine of half PM, and VM twice the chord RM or VX: therefore the square of the arc VM is always as the abscis VZ. The evolute is an equal cycloid, and the circles in Prop. VI. being as 1 to 4, the area of the cycloid is to that of its generating circle as 3 to 1. The properties of the cycloid as an isochronous and as a brachistochronous curve belong to mechanics, and it is demonstrated by writers on optics that its caustic is composed of two cycloids.

Case 2. Fig. 126. If the generating circle be supposed to become infinite while the base remains finite, the epicycloid will become the involute of a circle; and the fluxion of the curve being always, by Prop. III. cor. 1, to that of PM as PM to CP, its length SM will be a third proportional to IP and PM. Call CP, a, and PM, x, then the fluxion of SM is $\frac{x\dot{x}}{a}$; but the

rectangle contained by half PM and the fluxion of SM is the fluxion of the area PSM, or PSM $= \int \frac{xx\dot{x}}{2a} = \frac{x^3}{6a}$. The epitrochoid described by the point C of the generating plane will be the spiral of Archimedes, since CN is always equal to PM = PS = QV; and since the angular motion of CN and PM are also equal, the area CON = PSM = $\frac{x^3}{6a}$. Instead of the ellipsis of Prop. III., let PX be a parabola of which IP is the parameter, and continuing NM to X, the arc PX will be equal to CON. For making LQ = CP, it is well known that the fluxion of PX varies as XQ, or as PN, which represents the fluxion of CON. For the curvature, PY in Prop. IV. becomes = CP, and the radius is a third proportional to NZ and NP.

Case 3. Supposing now the generating circle to become again finite, but to have its concavity turned towards the basis, the same curve will be described as would be described by the rotation of a third circle on the same basis in a contrary direction, equal in diameter to the difference of those of the two first circles.

Case 4. If the circles be of the same size, with their concavities turned the same way, no curve can be described; but if the generating circle be still further lessened, a hypocycloid will be produced, of the same figure as that which would be described by a third circle equal in diameter to the difference of the two first. All the general propositions are equally applicable to hypocycloids with other epicycloids, as might easily have been understood from an inspection of the figures, if there had been room for a double series.

Case 5. Fig. 127. If the diameter of the generating circle be half that of the basis, the hypocycloid will become a right line, and the hypotrochoid an ellipsis. For since the angle PKM = 2PCS, PCM, being half PKM, coincides with PCS, and M is always in CS. Let GNL be the describing circle of the hypotrochoid, and join GNO, then NL is parallel, and ON perpendicular, to SC, and ON = HL, which is always to GO as CL to CG; therefore AN is an ellipsis: and the centre C will evidently describe a circle.

Fig 114.

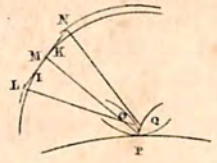


Fig 115.

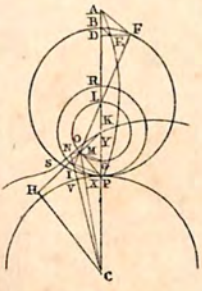


Fig 116.

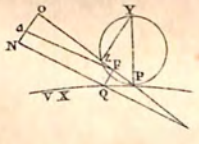
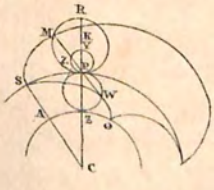


Fig 117.



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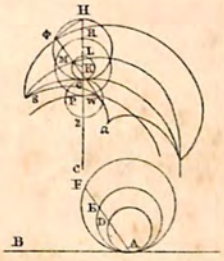

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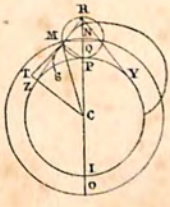
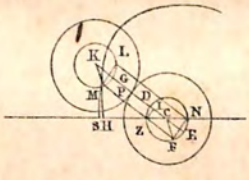


Fig 120.



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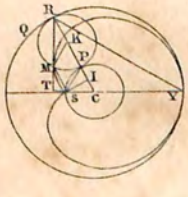

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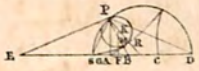

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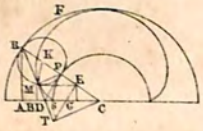

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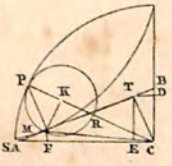
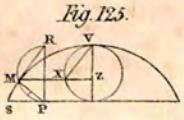


Fig 125.



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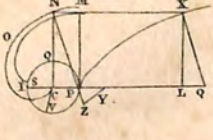
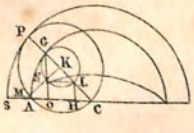


Fig 127.



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No. V.

AN ESSAY ON MUSIC.

From the British Magazine for October, 1800.

I. Of Music in general.

The agreeable effect of melodious sounds, not only on the human ear, but on the feelings and on the passions, is so universal and so powerful, as deservedly to excite the attention of the psychological philosopher. For what ultimate end a susceptibility for this peculiar pleasure has been implanted by nature in the mind, is not easy to be ascertained; but setting aside the well known pleasing sensation of a delicate titillation wherever the nerves are possessed of great sensibility, and the associations of an interesting voice, giving expression to poetical and impassioned diction, it is probable that the taste for all complicated and scientific music is wholly acquired.

Music may be considered as consisting of three component parts, rhythm, melody, and harmony. Rhythm is an agreeable succession of sounds considered with respect to the time of their whole duration. Melody is an agreeable succession in respect to the pitch, or the frequency of vibrations of each sound. Harmony is an agreeable combination of several sounds at the same time. It is evident that rhythm and melody are almost inseparable; but that harmony is by no means necessary to the existence of music. In the first place, it is easy to conceive that a love of rhythm, or of the periodical recurrence of the same or similar sensations at equal intervals of time, may be derived from the habit of a certain equality and recurrence in the motions of the body, such as walking, or in children who cannot yet walk, from the passive motion of gestation; this predilection for the return of customary sensations appears to

be an innate and fundamental tendency of the human system, to which physiologists and metaphysicians have been obliged ultimately to refer many properties, both of body and mind. But be this as it may, the love of rhythm, which is, perhaps, the lowest ingredient in musical taste, is, if possible, still more universal than the love of harmony and melody. Poetry, or rather metrical composition, is distinguished from prose only by the regularity of its rhythm; and the knowledge of metre and prosody, however high it may rank in the critic's estimation, is a subordinate and comparatively insignificant branch of musical science. The natural fondness for rhythm is the principal foundation of the pleasure of dancing, an amusement intimately connected with music, and no less popular. The rhythm of a musical composition is almost always at least twofold, often three or fourfold, consisting of subordinate divisions or bars, and periodical returns of larger members, either phrases or strains, containing equal numbers of those divisions. All this is perfectly natural, but perhaps, not so necessary to music as Mr. Walter Young, in his excellent essay, printed in the Edinburgh Transactions, appears to imagine; for those who are already experienced musicians are generally observed to delight in recitative, where the rhythm is almost entirely lost; and still more in fugues, where two or three series of rhythms, almost independent of each other, are carried on at the same time, one part beginning its subdivisions when another has made some progress, and a third is still to follow. But the pleasure derived from such compositions is, as Kirnberger has observed, more intellectual than sensual, arising in a great measure from the consciousness of being able to comprehend that which is "caviare to the general." Rhythm is generally marked in performance by a slight increase of force at the beginning of each subdivision or bar; sometimes, and in some instruments always, the change of sounds, in point of acuteness and gravity, or the interruption of the same sound, is a sufficient distinction; and sometimes, after the rhythm has already been firmly impressed on the mind, neither change of sound nor of strength is perpetually repeated; the imagination alone being sufficient to conceive the continuation of the rhythm: but

this constitutes a kind of *tempo rubato*, where the perception of measure is intentionally weakened or suspended. The Aeolian harp pleases indeed without rhythm, but the pleasure would soon be exhausted by repetition.

The next constituent part of music is melody. Melody may in some sense be said to please on the same principle as rhythm, the partiality of the mind to a regular recurrence of intervals: for though we have it not in our power to count the single vibrations of musical sounds numerically, yet we are evidently able to compare with ease such sounds as are related to each other in the simplest numerical ratios. For instance, if a treble and a tenor voice sing the same part, there is scarcely an ear so inaccurate as not to perceive their resemblance, which is produced by the recurrence of two vibrations of the treble note at the same interval of time with one of the tenor. The same love of order may easily be extended to the comparison of fifths and fourths, where the proportions are as two to three, and as three to four. This is enough to account in some degree for the pleasure derived from melody, or the succession of sounds bearing certain proportions to each other, in respect to gravity and acuteness; besides that the same intervals, which are most melodious in succession, are found also to form the most pleasing combination of harmony when cotemporary; for since the preceding sound is very frequently continued by reflection from surrounding objects so as to become cotemporary with the succeeding, and perhaps always remains fixed in the imagination, it is obvious that sounds, in order to be perfectly melodious, must also be harmonious. Add to this the impression generally made in infancy by the more or less melodious ditties of the nurse's voice, and the connexion of refined and chromatic melodies with the natural expression of the moans of grief, or the exclamations of joy: and from the union of all these causes it may easily be conceived from whence the love of melody, as an acquired faculty, may, without much difficulty, be derived.

The pleasure arising from harmony is not so simple and universal as that which is produced by a combination of rhythm and melody. Harmony requires for its execution and perception a greater degree of cultivation both in the performer

and in the hearer than melody alone. Cotemporary sounds may, from the due proportion of the times of their vibrations, give a similar pleasure to that of melody, when the mind, considering them in succession, finds them capable of a ready comparison. But the characteristic of harmony is the regular, and at the same time diversified, motion of the air, which arises from the combined vibrations, and which appears to be well calculated to produce the pleasure that the mind derives from the perception of symmetry. In this point of view, a concord may be considered as a single sound, distinguished from a discord by a superior quality of tone; in the same manner as the tone of the harmonica is more agreeable than that of a newsman's horn, as the note of a nightingale is sweeter than that of a frog, and a smooth rich voice more pleasing than a hoarse and nasal croaking. Thus the harshness and disagreeable quality of a single sound may also, on a more nice examination, be sometimes found to consist in a want of harmonious proportion in those secondary notes, which generally enter into its composition. This quality of sound, sometimes called its tone, register, colour, or *timbre*, might be considered as a fourth component part of music; it depends on the law by which the parts of the sounding body, and the particles of the air, are governed with respect to the velocity of their progress and regress in each vibration, or in different successive vibrations. No doubt, much of the pleasure derived from music depends on it; but as it is capable of little diversity on the same instrument, it is seldom considered in treating of the theory of music. The various combinations of the stops of the organ and harpsichord, the use of the harmonics of the harp and violin, the bowing nearer to or farther from the bridge, the application of muffles of various kinds, the change of the aperture of the lips in wind instruments, the choice of vowels and consonants more or less adapted to the powers of the voice; and in full pieces, the judicious introduction of different voices and instruments, as subservient to the general effect; all this relates to the quality of sound, and whoever adequately relishes the works of the great modern masters, will be fully competent to judge of its practical importance.

Be the causes what they may, natural or habitual, simple or complicated, it is certain that a very great majority of mankind experiences pleasure from music: this pleasure is a social pleasure, and, connected as it is with sentiment and passion, it is a rational pleasure. The pursuit of musical excellence, if properly conducted, amply exercises the faculties, at the same time that it forms a desirable variety, when intermixed with literary or professional employments. To call it an amusement only, betrays an ignorance of the nature and difficulty of the study; so far is the science of music from being of a light and superficial nature, that, in its whole extent, it is scarcely less intricate or more easily acquired than the most profound of the more regular occupations of the schools; and even practical perfection in music requires so much intense and laborious application, such a minute accuracy of perception, and so rapid an association of various sensitive ideas, with other ideas and mechanical motions, that it is inconceivable how men, who have no appearance of superior brilliancy in any other accomplishment, should be able to attain a conception and execution in music, which seem almost to require the faculties of a superior order of beings. An intemperate and dissipated attachment to music may indeed often be productive of evils; but probably the same individuals, who have been its victims, would have been equally idle and irregular if they had been destitute of this accomplishment. A considerable share of the pleasure of practical music arises from causes perfectly distinct from the sensual perceptions: the consciousness of having overcome difficulties, the laudable satisfaction of entertaining others, and the interest and emulation produced by a concurrence of others in the same pursuits; all these entirely outweigh the temporary amusement of the ear, and wholly remove the objection, which might be made, to the enervating effect of a continued devotion to pleasurable sensations. The ancient philosophers, with all the manliness and dignity of character to which they aspired, were not ashamed to consider music as an indispensable part of a liberal education; and Plato devotes three of the earlier years of his young citizens entirely to the study of the lyre: nor are we without examples in modern

times, of philosophers, and princes, and heroes, who have excelled as much in musical performances, as in literature and in arms.

II. Of the Origin of the Scale.

The first lyre, with three strings, is said to have been invented in Egypt by Hermes, under Osiris, between the years 1800 and 1500 before Christ. The second and third string were, perhaps, the octave and fifth of the first, or more probably its fifth and fourth; as it would be easy to sing the octave with the accompaniment of the primitive note only. The melody might be either always in unison with one of the strings, resembling a very simple modern bass part; or the intervals might be occasionally filled up by the voice, without accompaniment. We have, in modern music, a specimen of a pleasing air, by Rousseau, formed on three notes alone, the key-note with its second and third; but there can be little doubt that the earliest melodies must have had a greater compass than this; although some suppose the three strings of the oldest lyre to have been successive notes of the scale. The trumpet is said to have been invented about the same time: a little experience might have taught the Egyptians to produce from it the octaves, the 12th, 17th, 23rd, and other harmonics of the primitive sound, which are related to it in the ratio of the integers from 1 to 9, and the same sounds might have been observed by a delicate ear among the secondary notes of a long chord; and then, by descending three octaves from the 23rd, and two from the 17th, they might have added to their lyre the second and major third of the principal note. But it does not appear that this method ever occurred to the ancients: they seem rather to have attended to the intervals of the notes within the octave, than to the union of similar notes in the natural harmonics: and, besides, the series of natural harmonics would never have furnished a true fourth or sixth. It is uncertain when, or by whom, the fourth string was added: but the merit of increasing the number to seven is attributed to Terpander, about the year 700 before Christ, two centuries after Homer: although some persons have asserted that he

only brought the improvement from Egypt, and that Hermes was also the inventor of the lyre with seven strings. Pythagoras, or Simonides, about the year 500 added an eighth, and Timotheus a ninth string: the number was afterwards extended to two octaves; and Epigonus is said to have used a lyre of forty strings, or rather a harp, as he played without a plectrum: but the theory of the ancient music soon became more intricate than interesting. The lyre of eight strings comprehended an octave, corresponding pretty accurately with the notes of our natural scale, beginning with *e*: the key-note was *a*, so that the melody appears to have borne usually a minor third, which has also been observed to be the case in the airs of most uncultivated nations; but there was a considerable diversity in the manner of tuning the lyre, according to the great variety of modes and genera that were introduced. These modes were of a nature totally different from the modern modulations into various keys, but they must have afforded a more copious fund of striking, if not of pleasing melodies than we have at present. In some of the genera, intervals of about a quarter tone were employed; but this practice, on account of its difficulty, was soon abandoned; a difficulty which is not easily overcome by the most experienced of modern singers; although some great masters have been said to introduce a progression of quarter tones, in pathetic passages, with surprising effect. The tibia of the ancients, as it appears evidently from Theophrastus, although not from the misinterpretations of his commentators and of Pliny, had a reed mouth-piece about three inches long, and therefore was more properly a clarinet than a flute; and the same performer generally played on two at once, and not in unison. Pollux, in the time of Commodus, describes, under the name of the Tyrrhene pipe, exactly such an organ as is figured by Hawkins, composed of brass tubes, and blown by bellows: nor does he mention it as a new discovery: it appears, from other authors, to have been often furnished with several registers of pipes; and it is scarcely possible that the performer, who is represented by Julian as having considerable execution, should have been contented without occasionally adding harmony to his melody. That the

voice was accompanied by thorough bass on the lyre, is undeniably proved by a passage of Plato: and that the ancients had some knowledge of singing in three parts, is evident from Macrobius. Martini, who is one of the strongest opponents of that opinion, which attributes to the ancients a knowledge of counterpoint, observes that "they allowed no concords but the octave, fourth, and fifth, or at most very rarely the third; yet they were not without a knowledge of concord of harmonious parts. It is known with certainty, that two parts, whether vocal or instrumental, or mixed, besides unison, performed at the same time the same melody, either always in octaves, or probably always in fifths, or always in fourths; which was called a symphony: perhaps also, they changed in the course of the performance from one interval to another, and this might be done by more than two parts at the same time." It is not improbable that this statement may be accurate: nor is it necessary to suppose a very exquisite and refined skill in the intricacies of composition, to produce all the effects that have with any probability been attributed to music. It is well known that Rousseau and others have maintained, that harmony is rather detrimental than advantageous to an interesting melody, in which true music consists; and it may easily be observed, that an absolute solo, whether a passage or a cadence, is universally received, even by cultivated hearers, with more attention and applause, than the richest modulations of a powerful harmony.

The minor scale being the most commonly used by the ancients, it was natural for Pope Gregory, who in the year 600 is said to have marked the notes by the Roman letters, to begin with A, the key-note of that scale: although if, as there is some reason to suppose, the B was originally flat, A was not the key-note, but its fifth, until the B natural was introduced, and denoted by a square b instead of a round one. By degrees the chromatic scale was filled up, and the five added intervals were denoted by the letter belonging to the note above them, with the addition of the round b, or by the note below, with the addition of four lines crossing each other, implying a half note, as composed of four commas. A simple cross would,

however, at present, be much more convenient, as more readily distinguishable from the square b, which is used to signify a natural note, in opposition to these flats and sharps. This is the historical account of the origin of the scale; but, according to the modern theory and practice of music, the subject may be more easily understood, by beginning with an explanation of the major scale.

III. Practical Application of the Scales.

The simplest proportions of two sounds to each other, next to unison, is when the frequency of their vibrations is related as one to two: such sounds bear a very strong resemblance to each other, and when named, they are denoted by the same letter, and are only distinguished by the appellations in alt, in altissimo, on the one side, and double, and double double, on the other. The Germans, with great propriety, make use of small letters or capitals, with one, two, or more lines over or under them. The note marked by the tenor cleff is called $\overline{c}$, the octaves above $\overline{\overline{c}}$, $\overline{\overline{\overline{c}}}$, as far as six lines, which is, perhaps, the highest note used in music: the octaves below $\overline{c}$, are c, C, $\underline{C}$, $\underline{\underline{C}}$: $\underline{\underline{\underline{C}}}$ is probably not audible, vibrating but eight times in a second. C with six lines below it, would denote a sound, of which the complete vibrations should last precisely a second. The series of natural notes is this, $\underline{A}$, $\underline{B}$, C, D, E, F, G, A, B, c, d..b, $\overline{c}$, $\overline{d}$...The subjoined table will show the absolute frequency and the dimensions of each vibration of the octaves of c, and the length of the simplest organ pipe that produces it: but, according to the different temperature of the air, and the pitch of the instruments, these numbers may vary somewhat from perfect accuracy: and it must be observed, that the usual pitch of concerts, in London, is somewhat higher than this standard; and in Germany, perhaps a little lower.

Sound moves in a second 1130 feet.			
Note.		Vibrations in a second.	Length of open pipe in feet.
C_6		1	565.00
C_5		2	282.50
C_4		4	141.25
C_3		8	70.62
C_2	audible	16	35.31
C	New P. Forte.	32	17.66
$\overline{C}$		64	8.83
c		128	4.41
$\overline{c}$		256	2.21
$\overline{\overline{c}}$		512	1.10
c^3		1024	.55
c^4		2048	.28
c^5		4096	.14
c^6		8192	.07

Any sound may be assumed at pleasure for the primitive or standard note of a piece of music, and is then denominated the key note; and the idea of this note is perpetually impressed on the mind in all simple compositions, both from its frequent recurrence, and from the relation that all the other sounds bear to it. C being the key note of the scale called natural, we shall consider it as the foundation of the scale. The next in importance is the fifth, G, which, for various reasons, is intimately connected with the key note. The first reason is, that it constitutes the most perfect melody and harmony with C, since every alternate vibration of C coincides with every third of G; the second is, that an attentive ear may almost always distinguish the fifth, at least its octave, the 12th, whenever any instrument sounds C; it being one of those secondary sounds which are called natural harmonics, and which may generally be observed, in the proportion of the natural numbers, as far as

twenty or more, but which have not hitherto been completely explained: thirdly, a stopped pipe, if blown forcibly, springs immediately from C to g, and an open pipe first to c, and then to g. The interval, between C and G, is most naturally divided by the note E, which answers to the number 5, when C and G are represented by 4 and 6, and which is found among the natural harmonics both of chords and pipes. These three notes constitute the harmonic triad, or common accord, in the major scale, which is the most perfect, or rather the only perfect harmony. But the intervals are still much too large for melody, and require a further subdivision; we now therefore take the fifth below instead of above the key, or its octave, the fourth above, F, which is to C as 4 to 3: this sound is nowhere found among the natural harmonics of C, but C is the most distinguishable of its harmonics, and therefore the relation is nearly the same. The scale is completed by filling up the perfect triads of G and F: the fifth of G furnishing D, the second of the key, which is also the ninth natural harmonic of C; the third of G, the seventh, B, which is the fifteenth harmonic of C; and the third of F being the sixth of the key, A, which is neither among the harmonics of C, nor has c among its harmonics. Hence we have a second table, in which the proportions of the length of a chord, or pipe, producing the various sounds, are detailed, and the place among the principal natural harmonics of the key annexed.

Notes.	Proportions.	Natural Harmony.
Key C	1	1
2d D	$\frac{8}{9}$	9
3d E	$\frac{4}{5}$	5, 10
4th F	$\frac{3}{4}$	0, ($\frac{1}{3}$)
5th G	$\frac{2}{3}$	3, 6, 12
6th A	$\frac{3}{5}$	0
7th B	$\frac{3}{15}$	15
8th c	$\frac{1}{2}$	2, 4, 8, 16

Now, when two or more perfectly harmonious parts are performed together, they must necessarily be found all in the same triad, C, E, G; G, B, D; or F, A, C; and the succession of these triads, in various forms, is sufficient for the accompaniment of any simple melody. A regular melody always terminates by an ascent or descent of one degree to the key note; the last note but one must therefore be always B or D: and both of these being in the triad of G, G is called the governing note, or the dominant of C; and F, being in the same manner governed by C, is called its subdominant. And it is usual, in all regular compositions of any length, to depart for a short time from the principal harmony of the key note, and to modulate into the key of the dominant, then to return, and to modulate for a still shorter time into the subdominant, before the final close in the tonic or key note. It is necessary, therefore, for greater variety, to complete the scale of the dominant, as well as that of every other note which may be occasionally introduced as a principal key note; but to do this with mathematical accuracy, in the same proportions as have been explained, would be practically impossible, and even theoretically inconvenient: hence arises the necessity of tempering some intervals, to make the others more tolerable, without too much increasing the number of sounds. It has been found sufficient in practice, to add five notes to the seven which have been enumerated; but the best proportions of these have not yet been absolutely determined: some have made all the twelve intervals equal: others have left the whole scale of C perfect: others again have taken a middle path, and have introduced a slight imperfection into this key, in order to make the neighbouring ones the less disagreeable. The least circuitous introduction of these notes is shown in the third table, together with the proportion that they bear to C when thus considered. They are denominated nearly in the German manner, the addition of the syllable "is" signifying what the English call sharp, and the French *diése*, and that of "es," flat or *bémol*.

Notes.	Relations.	Proportions.
Fis	as 7th of G	$\frac{8}{45}$
Bes	as 4th of F	$\frac{6}{160}$
Cis	as 7th of D	$\frac{128}{135}$
Ees	as 4th of Bes	$\frac{27}{32}$
Gis	as 3rd of E	$\frac{16}{25}$

But a still greater variety being required than these major scales afford, it has been found, that the interval of a fifth may be agreeably, though somewhat less harmoniously, divided, by placing the minor third below, instead of above, the major; so that C may be to E as E was to G, and consequently E to G as C was to E. The E, thus depressed to $\frac{5}{6}$, differs but by a comma, or in the ratio of 80 : 81, from the Ees found above, as the fourth of Bes; therefore the same string serves for both notes; and the scale becomes C, D, Ees, F, G, A, B, C; which is the ascending minor scale, the A and B being retained as leading best towards the key note, and the major triad of the dominant being therefore necessary to the cadence. But in descending, the triad of the subdominant F may conform to the character of the minor mode, and Aes is substituted for A; and most frequently Bes for B, as dividing the interval from C to Aes more equally and more melodiously.

Thus we have a pretty comprehensive view of the most usual practical relations of all the notes to each other. Their use as discords is somewhat more complicated, and would lead further into the science of music than is consistent with the nature of so summary a view. But it may be remarked in general, that by far the most common discord is the note which constitutes the distinction of the scale of the key from that of its dominant; for instance, F with the triad of G, which is called the accord of the flat seventh of G; and F, not being in the scale of G, is considered as a regular preparative to the final accord of C; in which that part or instrument by which the F is introduced, must necessarily descend to E, the third of the key. The second kind of discords are suspended discords, when one or

more notes of any preceding accord are continued after the commencement of a different harmony in other parts of the composition. The third, which is rare, and less universally adopted, consists in an anticipation of a subordinate note of an accord which is to follow, as in the case of the added sixth of the French school. The fourth kind are passing discords, where a note, forming only a melodious step between two others, is inserted without any regard to its harmonious relations.

IV. Of the Terms expressive of Time.

The notation of music, as it has been established for more than two centuries, is in general admirably adapted for its purpose: but there is one great deficiency, which might very easily be remedied, and that is, the total omission of any character expressive of the absolute duration of each note, however accurately the relative value of the notes may be prescribed. It is true, some little allowance must be made for the execution of the performer, and for the habits of the audience; but this is no reason why time might not be much more accurately noted, than by the vague terms which are usually adopted. It would be easy to prefix to each movement a number, signifying how many bars are to be performed in a minute, which might at first be ascertained by the help of a stop watch, and would soon become perfectly familiar both to composers and performers, even without this assistance. According to Quanz, the number which should be substituted for *Allegro assai*, in common time, is about 40; for *Allegretto*, 20; for *Larghetto*, 10; and for *Adagio assai*, 5. But it is usual to perform modern music much more rapidly than this; or at least the style of composition is so changed, that the terms are very differently applied. An allegro, or even an allegretto, in common time, without semiquavers, is often performed as fast as 60; seldom slower than 30.

A very superficial attempt to affix a determinate meaning to the words denoting musical time, may be seen in the table subjoined; which, if it were more completely and accurately filled up, might be of considerable use to young musicians; although it will appear, from inspection of this table, that composers have

Terms.	C	2/4	3/4	6/8	3/8
Prestissimo	As fast	as	you	can.	
Presto		100.		80, Haydn.	
Presto ma non troppo		90, Haydn.			
Allegro assai	40, Quanz	80, Pleyel	70, Haydn.		
Allegro molto		100, Haydn.			
Allegro vivace			60, Pleyel	50, Pleyel.	
Allegro	25, Handel. 45, Mozart. 40, 50, 60, Pleyel	100, 120, Haydn. 75, 80, Pleyel.	65, Haydn.		
Allegro non troppo		60, Pleyel		50, Pleyel.	
Allegro moderato	40, Haydn	70, Mozart.			
Vivace assai		100, Haydn		60, Haydn.	
Vivace			60, Haydn.		
Spiritoso		90, Haydn.			
Menuetto allegro molto			75, Haydn.		
Menuetto allegro			70, Haydn; Pleyel.		
Menuetto allegretto			55, Haydn.		
Menuetto moderato			50, Haydn; Mozart.		
Allegretto	20, Quanz				45, Pleyel.
Allegretto grazioso	50, Mozart.				
Moderato				30, Pleyel.	
Maestoso	19.				
Larghetto	10, Quanz.				
Andante grazioso				30, Mozart.	
Andante		35, Haydn; Pleyel. 50, Pleyel.		30, Mozart.	
Andante cantab.			20, 25, Mozart.		
Largo cant.	20, Haydn.				
Adagio non troppo	17, Haydn	27, 30, Pleyel			20.
	7, Corelli		14, Haydn	10, 12, Mozart.	
	15, Scoz			10, Pleyel.	

hitherto employed those terms in very indefinite significations. But it must be confessed, that much latitude must necessarily be left for the ear and taste of a judicious performer, and that it is impossible for human art to describe on paper every delicacy of finished execution.

If we choose to compare the time, occupied either by a bar, or by any of its parts, with the vibrations of a pendulum, we may easily do it by means of the following table, which shows the number of vibrations in a minute, corresponding to pendulums of different lengths, expressed in inches.

Length.	Vibrations.	Length.	Vibrations.	Length.	Vibrations.
4	187	10	118	30	68
5	167	12	107	35	63
6	153	15	97	40	59
7	142	20	84	50	53
8	132	25	75	60	47
9	125				

No. VI.

A LETTER TO MR. NICHOLSON, PROFESSOR OF NATURAL PHILOSOPHY IN THE ROYAL INSTITUTION, RESPECTING

SOUND AND LIGHT,

AND IN REPLY TO SOME OBSERVATIONS OF PROFESSOR ROBISON.

From Nicholson's Journal for August, 1801.

SIR,

IN the Supplement to the Encyclopædia Britannica are inserted several excellent articles by Professor Robison of Edinburgh; one of them appears to require some public notice on my part, and I consider your valuable Journal as the most eligible channel for such a communication, especially as you have lately done me the honour of reprinting the paper which gave rise to the Professor's animadversions. But in the first place I shall beg leave to recall the attention of your readers, by a summary enumeration, to the principal positions which I have in that paper endeavoured to establish.

1. Sound, as transmitted through the atmosphere, consists in an undulatory motion of the particles of the air, Sect. III.* This is generally admitted; but as the contrary has even very lately been asserted, it is not superfluous to have decisive evidence of the fact. Professor Robison's experiment with a stopcock furnishes an argument nearly similar.

2. A current of air, forced by a moderate pressure through a cylindrical pipe, diverges the less as its velocity is less, Sect. II.†

3. At a certain point the divergency of such a current increases suddenly, and the current mixes with the surrounding air, Sect. II.‡

* Supra, p. 71. † Supra, p. 69.

4. So far is such a motion from spreading equally in all directions, that on every side of the current the air is urged more towards it than from it, Sect. II.

5. Sound, admitted through an aperture, does not by any means diverge equally in all directions, and is probably very weak except in directions nearly rectilinear. From position 2 and 4, and from experience Sect. V I.

6. Sound probably decays in the duplicate ratio of the distance, Sect. VII.†

7. A similar blast of air produces nearly a similar sound, in organ pipes properly commensurate, Sect. VIII.‡

8. Light is probably the undulation of an elastic medium, Sect. X.§

A. Because its velocity in the same medium is always equal.

B. Because all refractions are attended with a partial reflection.

C. Because there is no reason to expect that such a vibration should diverge equally in all directions, and because it is probable that it does diverge in a small degree in every direction.

D. Because the dispersion of differently coloured rays is no more incompatible with this system than with the common opinion, which only assigns for it the nominal cause of different elective attractions.

E. Because refraction and reflection in general are equally explicable on both suppositions.

F. Because inflection is as well, and it may be added, even much better explained by this theory.

G. Because all the phenomena of the colours of thin plates, which are in reality totally unintelligible on the common hypothesis, admit a very complete and simple explanation by this supposition. The analogy which is here superficially indicated, will probably soon be made public more in detail; and will also be extended to the colours of thick plates, and to the fringes produced by inflection, affording, from Newton's own

* Supra, p. 73. † Supra, p. 75. ‡ Supra, p. 76.
§ Supra, p. 78.

elaborate experiments, a most convincing argument in favour of this system.*

9. The particles of air may be jointly actuated by two or more sounds; and in this case, the several motions are to be added or subtracted, in order to find the actual joint motion, Sect. XI.†

10. The grave harmonic produced by a major third is accompanied by a very audible twelfth. This circumstance is explained, and the effect of subordinate notes and subaltern stops on the quality of sounds is shown by figures, Sect. XI.

11. A noise returning every second, if audible, would be a C. From Sauveur; with an experiment, Sect. XII.‡

12. A chord retains always the form of its initial vibration. From experiments, in favour of Euler's theorem, against the simple harmonic curve, Sect. XIII.§

13. The vibration of a chord is scarcely ever performed in the same plane. Its revolutions and its subordinate vibrations may be rendered distinctly visible under the microscope, Sect. XIII.

14. If a chord be inflected at any point of aliquot division, the harmonic secondary note, corresponding to that division, will not be audible; an experiment contradictory to some theories of the origin and of the inseparable nature of harmonic sounds, Sect. XIII.

15. The human voice is analogous to the organ pipe denominated from it, which consists of a tongue piece without any commensurate tube: and the falsetto is probably formed by the upper orifice of the trachea, assuming the functions of the glottis, Sect. XV.‖

16. A temperament of progressive imperfection is the most convenient for practical music, and is easily approximated by tuning six perfect, and six equally imperfect fifths, Sect. XVI.¶

From the detached nature of the subjects which I have here

* It appears from the enunciation of the last of these propositions that Dr. Young was already in possession of the principle of interference, and of some of its most important applications; they were announced in his celebrated paper "On the Theory of Light and Colours," (Infra, No. VII., p. 140,) which was read before the Royal Society on the 12th of November following.—*Note by the Editor.*

† Supra, p. 83. ‡ Supra, p. 85. § Supra, p. 86. ‖ Supra, p. 91. ¶ Supra, p. 93.

enumerated, and the imperfect state of those branches of the mathematics to which they refer, it would have been in vain to attempt a very perspicuous and detailed discussion of them. My researches on these subjects have been much interrupted, and probably will not be very shortly resumed; but if they be of no further use to any person, I shall not think my labour lost; for I flatter myself that the inferences which they have led me to draw respecting the theory of colours will throw new light on all the most interesting parts of optics, while, by a comparison with the obvious inferences from Dr. Herschel's important discoveries, they will also lead to some material illustrations of the phenomena of heat.

I shall now trouble you with some remarks in reply to Professor Robison: the passage to which I allude is this:—

"We are surprised to see this work of Dr. Smith greatly undervalued by a most ingenious gentleman in the Philosophical Transactions for 1800,* and called a large and obscure volume, which leaves the matter just as it was, and its results useless and impracticable. We are sorry to see this, because we have great expectations from the future labours of this gentleman in the field of harmonics; and his late work is rich in refined and valuable matter. We presume humbly to recommend to him attention to his own admonitions to a very young and ingenious gentleman,† who, he thinks, proceeded too far in animadverting on the writings of Newton, Barrow, and other eminent mathematicians." Encyclop. Brit., Suppl., Art. Temperament, p. 652. (Works, Vol. iv. p. 429.)‡

* Supra, p. 93. † Supra, p. 102.

‡ This admirable Essay, containing a most luminous exposition of the Theory of Music, is given in the fourth volume of Sir David Brewster's edition of Dr. Robison's Works, p. 376. Similar remonstrances against Dr. Young's estimate of the character of Dr. Smith's Harmonics were addressed to him by Mr. John Gough (Manchester Memoirs, Vol. V., "On the Theory of Compound Sounds"), to which he replied in Nicholson's Journal for Aug. 1802. The passage in Dr. Smith's work which gave rise to his observations was the following: "Different particles of the air at the ear will keep moving constantly opposite ways at the same time, And in so rare a fluid as air is, where the intervals of the particles are eight or nine times their diameters, there seems to be room enough for such opposite motions without impediment: especially as we see the like motions are really performed in water, which in an equal space contains eight or nine hundred times as many such particles as air does. For when it rains upon stagnating water, the circular waves propagated from different centres appear to intersect and pass through or over each other, even in opposite directions, without any visible alteration in their circular figure, and therefore without any sensible alteration of their motions."—Harmonics, 1759, p. 105.—*Note by the Editor.*

According, therefore, to the author of this article, I have, in the first place, taken the liberty of giving severe advice to a young mathematician who had never asked it; secondly, this advice is equally applicable to my own presumption; and thirdly, Dr. Smith's Treatise on Harmonics is a work entitled to the highest praise.

I did, in fact, endeavour to show that the gentleman in question had overlooked the labours of some former authors relative to his subject, but I accompanied my remarks with nothing like admonition. I have read Dr. Smith's work with attention, and I imagine, from the polite manner in which Professor Robison is pleased to speak of my essay, he will not hesitate to allow that I have understood it. I took it up with great expectations; those expectations having been completely disappointed, I thought it right to state my cool and unprejudiced opinion of its merits, in order to prevent a similar disappointment in others. It is impossible, therefore, that an "attention" to any "admonitions" of a general nature, wherever they may be found, can influence such an opinion; and so far only as I am supposed to be an incompetent judge on the subject of harmonics can it be asserted that it was either blameable or superfluous for me to express that opinion. As a mathematician, and an optician, I value Dr. Smith highly; but I must still beg leave to affirm that his whole book of harmonics contains far, far less information than either of the articles Temperament and Trumpet, in the Supplement to the Encyclopædia.

I do not mean it to be understood that this work is so contemptible as not to contain the least particle of important matter; but it appears to me that its errors counterbalance its merits. The only improvement on which Professor Robison himself seems to set a high value, is the application of the phenomena of beats to tuning an instrument; on the other hand, I conceive that the misstatement, relative to the non-interference of different sounds, is an inaccuracy which far outweighs the merit of Dr. Smith's share of that improvement. I have asserted that Dr. Smith has written a large and obscure volume, which, for every "purpose, but for the use of an impracticable instrument, leaves the whole subject of temperament precisely where

it found it;" and that "the system proposed for his changeable harpsichord is neither in that, nor in any other form, capable of practical application." Professor Robison, on the contrary, says, "We do not see how it can be disputed that Dr. Smith's theory of the beating of imperfect consonances is one of the most important discoveries, both for the practice and the science of music, that have been offered to the public. We are inclined to consider it as the most important that has been made since the days of Galileo. We are obliged to call it his discovery. Mersennus, indeed, had taken particular notice of this undulation of imperfect consonances, and had offered conjectures as to their causes; conjectures not unworthy of his great ingenuity. Mr. Sauveur also takes a still more particular notice of this phenomenon, and makes a most ingenious use of it for the solution of a very important musical problem." P. 652 and 651. (Works, p. 429.)

Why then are we obliged to call it Dr. Smith's discovery, or indeed any discovery at all? Sauveur had already given directions for tuning an organ pipe, by means of the rapidity of its beating with others. Mém. de l'Ac. 1701, 475, ed. Amst. Dr. Smith ingeniously enough extended the method; but it appears to me that the extension was perfectly obvious, and wholly undeserving of the name either of a discovery or of a theory. If Professor Robison thinks otherwise, there is nothing further to be said; but, in all probability, Dr. Smith considered this improvement as constituting a very small part of the merit of his treatise. No doubt an organ may be more accurately tuned by counting the beats than by any other method, although it may be questioned whether the advantage of counting the absolute frequency of the beats will ever practically compensate the tediousness of the process.

It remains to be considered whether Dr. Smith's changeable harpsichord is, or is not, an impracticable instrument; for, whatever Signor Doria might exclaim, Dr. Smith himself does not recommend his scale for common use. It is the opinion of many unprejudiced practical persons that all occasional introduction of different semitones is perfectly impracticable; and some who have heard the effect of Dr. Smith's

instrument have declared that to them it was by no means agreeable. And, indeed, if we pay sufficient attention to the passages and modulations of the greatest composers, we shall be convinced that, granting all possible dexterity in the performer, it would be absolutely impracticable to adapt them to an instrument so different from that for which they were composed as Dr. Smith's is from the common harpsichord. It may easily be conceived that an organ, very correctly tuned, as Mr. Watt's probably was, for a particular key, might appear "sopra modo bellissimo" in that key; but the sequel of the story shows literally what Dr. Smith has allowed, that his temperament is inapplicable to our instruments, since it was utterly impossible to sing with it in the key of Ees or E flat, a key of exceedingly frequent occurrence. I have been informed, on the best authority, that Dr. Smith restricted the organist of Trinity College to such keys and modulations as were best suited to the system by which the organ was tuned; and that organ, as well as the instruments which were made for Dr. Smith, has long been tuned according to the more common method.

I spoke of Dr. Smith's system with flattened major thirds as of no value, not with regard to its intrinsic merits, but because it was not intended for any instrument in common use; since, in these instruments, the difficulty is not so much how to divide the imperfection among the thirds and fifths of the same scale as to proportion properly the imperfections of the thirds of different keys. Yet I do not mean it to be understood that I can agree to the solidity of those foundations on which Dr. Smith has built his system for a single scale; although to Stanley and to Doria it might be pleasing, because its imperfections are far too small to offend the ear. Professor Robison justly observes that different persons differ exceedingly in their estimation of the effect of the same temperament on different concords, and that much of this arises from their different dispositions; it appears, therefore, that Dr. Smith was too precipitate in laying down his principle for the comparison of the effects of temperament.

With respect to the system which I have proposed, Professor Robison thinks that the temperaments of several of the thirds

which occur frequently are much too great. If we wish to form a judgment of any system of temperament, it must be by comparison with some other. It does not appear with what system Professor Robison would wish the comparison to be made, but he rather seems to incline to the equal temperament, although he gives directions for tuning by another. At any rate, no temperament of an interval can be said to be much too great, unless it be greater than that of the same interval in the system of equal temperament; for if any interval be made more perfect than this, some other similar interval must be as much less perfect. In my system the only thirds, perceptibly greater than those of the equal temperament, are the major thirds on E, Aes, B, C sharp, or Cis, and Fis, and the minor on C, Cis, F, Gis, Bes, and Ees. Of these none can be said to occur frequently except the major third on E, and the minor on C. The sixths require no separate consideration. Now, since the minor chord is intended to be less completely harmonious than the major, its character will be by no means materially impaired by this imperfection, which it would be somewhat difficult to remove. The third on E is not sharp enough to be very offensive; but, in compliance with the usual practice of making this third somewhat more perfect than the intervals of Aes and C, I have, in the method recommended for common use, made it equal to the third of the equal temperament. The directions given for tuning in § 68 and in § 80 of the article are liable to far greater objections. For instance, the temperament of the IIIds on Aes and Fis, in the latter, is about .00880, or more than a comma and a half, which Professor Robison will readily allow to be "much too great" for any thirds; since he has asserted, with Dr. Smith and others, that the error of a comma would be intolerable. Mr. Maxwell has, however, very decidedly proved, in his Essay on Tune, that the greatest harmonists, Corelli, Tartini, and Giardini, have admitted very frequently the error of a comma in their most refined compositions. And I have the authority of several celebrated performers on stringed and wind instruments for asserting that they take of choice the characteristic semitone, leading into the key note, considerably sharper than the same note is tuned on

any keyed instruments, making an imperfection of nearly two commas, in the relation as third of the dominant, which is the fundamental note of the chord; while, in the mean time, our theorists have been labouring, by the most complicated contrivances, to introduce notes into keyed instruments, which shall have exactly a contrary effect, by making the ascending semitone as wide a step as possible. On asking very lately the opinion of a practical musician of great eminence, and one who, in every respect, does honour to his profession, he decidedly agreed in the superiority of such a diminished semitone, and observed that the key of E derived a very elegant character from the usual method of tuning Dis as Ees, a minor third to C: hence the IIIds on Ees and G being very little tempered, the IIId on the dominant B must be about a comma and half too sharp. The fact is, that in this case the harmony is somewhat impaired, in order to improve the melody. The semitone is considered only in its relation to the key note: the interval of 15 to 16 is far too small to be distinctly conceived as commensurate, it possesses, therefore, no melody in virtue of the perfection of its ratio; and a certain elegance of expression is added, by approaching to the natural and colloquial ascent of a voice by imperceptible degrees. It must, however, be confessed that some excellent musicians prefer a purer harmony; and in this, as in all other matters of taste, considerable latitude must be allowed for the habits and predilections of individuals.

I am, Sir,
With great respect,
Your obedient humble servant,

THOMAS YOUNG.

No. 48, Welbeck-street,
13 July, 1801.

No. VII.

ON THE THEORY OF

LIGHT AND COLOURS.

From the Philosophical Transactions for 1802, p. 12.

A BAKERIAN LECTURE.

READ NOV. 12, 1801.

ALTHOUGH the invention of plausible hypotheses, independent of any connexion with experimental observations, can be of very little use in the promotion of natural knowledge; yet the discovery of simple and uniform principles, by which a great number of apparently heterogeneous phenomena are reduced to coherent and universal laws, must ever be allowed to be of considerable importance towards the improvement of the human intellect.

The object of the present dissertation is not so much to propose any opinions which are absolutely new, as to refer some theories, which have been already advanced, to their original inventors, to support them by additional evidence, and to apply them to a great number of diversified facts, which have hitherto been buried in obscurity. Nor is it absolutely necessary in this instance to produce a single new experiment; for of experiments there is already an ample store, which are so much the more unexceptionable, as they must have been conducted without the least partiality for the system by which they will be explained; yet some facts, hitherto unobserved, will be brought forwards, in order to show the perfect agreement of that system with the multifarious phenomena of nature.

The optical observations of Newton are yet unrivalled; and, excepting some casual inaccuracies, they only rise in our estimation as we compare them with later attempts to improve

on them. A further consideration of the colours of thin plates, as they are described in the second book of Newton's Optics, has converted that prepossession which I before entertained for the undulatory system of light, into a very strong conviction of its truth and sufficiency; a conviction which has been since most strikingly confirmed by an analysis of the colours of striated substances. The phenomena of thin plates are indeed so singular, that their general complexion is not without great difficulty reconcileable to any theory, however complicated, that has hitherto been applied to them; and some of the principal circumstances have never been explained by the most gratuitous assumptions; but it will appear, that the minutest particulars of these phenomena are not only perfectly consistent with the theory which will now be detailed, but that they are all the necessary consequences of that theory, without any auxiliary suppositions; and this by inferences so simple, that they become particular corollaries, which scarcely require a distinct enumeration.

A more extensive examination of Newton's various writings has shown me that he was in reality the first that suggested such a theory as I shall endeavour to maintain; that his own opinions varied less from his theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a similar form in his own works; and this by no less a mathematician than Leonard Euler, whose system of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huygens, and Malebranche.

Those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the Newtonian approbation, will probably be disposed to bestow on these considerations so much the more of their attention, as they appear to coincide more nearly with Newton's own opinions. For this reason, after having briefly stated each particular position of my theory, I shall collect, from Newton's various writings, such passages as seem to be the most favourable to its admission; and although I shall quote some papers which may be

thought to have been partly retracted at the publication of the Optics, yet I shall borrow nothing from them that can be supposed to militate against his maturer judgment.

Hypothesis I.

A luminiferous ether pervades the universe, rare and elastic in a high degree.

Passages from Newton.

"The hypothesis certainly has a much greater affinity with his own," that is, Dr. Hooke's, "hypothesis, than he seems to be aware of; the vibrations of the ether being as useful and necessary in this as in his." (Phil. Trans., Vol. VII. p. 5087. Abr., Vol. I. p. 145. Nov. 1672.)

"To proceed to the hypothesis: first, it is to be supposed therein, that there is an ethereal medium, much of the same constitution with air, but far rarer, subtler, and more strongly elastic. It is not to be supposed that this medium is one uniform matter, but compounded, partly of the main phlegmatic body of ether, partly of other various ethereal spirits, much after the manner that air is compounded of the phlegmatic body of air, intermixed with various vapours and exhalations: for the electric and magnetic effluvia, and gravitating principle, seem to argue such variety." (Birch, Hist. of R. S., Vol. III. p. 249, Dec. 1675.)

"Is not the heat (of the warm room) conveyed through the vacuum by the vibrations of a much subtler medium than air? —And is not this medium the same with that medium by which light is refracted and reflected, and by whose vibrations light communicates heat to bodies, and is put into fits of easy reflection, and easy transmission? And do not the vibrations of this medium in hot bodies contribute to the intenseness and duration of their heat? And do not hot bodies communicate their heat to contiguous cold ones, by the vibrations of this medium propagated from them into the cold ones? And is not this medium exceedingly more rare and subtle than the air, and exceedingly more elastic and active? And doth it not readily pervade all bodies? And is it not, by its elastic force,

expanded through all the heavens?—May not planets and comets, and all gross bodies, perform their motions in this ethereal medium?—And may not its resistance be so small as to be inconsiderable? For instance, if this ether (for so I will call it) should be supposed 700,000 times more elastic than our air, and above 700,000 times more rare, its resistance would be about 600,000,000 times less than that of water. And so small a resistance would scarce make any sensible alteration in the motions of the planets in ten thousand years. If any one would ask how a medium can be so rare, let him tell me how an electric body can by friction emit an exhalation so rare and subtle, and yet so potent?—And how the effluvia of a magnet can pass through a plate of glass without resistance, and yet turn a magnetic needle beyond the glass?" (Optics, Qu. 18, 22.)

Hypothesis II.

Undulations are excited in this ether whenever a body becomes luminous.

Scholium. I use the word undulation, in preference to vibration, because vibration is generally understood as implying a motion which is continued alternately backwards and forwards, by a combination of the momentum of the body with an accelerating force, and which is naturally more or less permanent; but an undulation is supposed to consist in a vibratory motion, transmitted successively through different parts of a medium, without any tendency in each particle to continue its motion, except in consequence of the transmission of succeeding undulations, from a distinct vibrating body; as, in the air, the vibrations of a chord produce the undulations constituting sound.

Passages from Newton.

"Were I to assume an hypothesis, it should be this, if propounded more generally, so as not to determine what light is further than that it is something or other capable of exciting vibrations in the ether: for thus it will become so general and comprehensive of other hypotheses, as to leave

little room for new ones to be invented." (Birch, Vol. III. p. 249, Dec. 1675.)

"In the second place, it is to be supposed that the ether is a vibrating medium like air, only the vibrations far more swift and minute; those of air, made by a man's ordinary voice, succeeding one another at more than half a foot, or a foot distance; but those of ether at a less distance than the hundred thousandth part of an inch. And, as in air, the vibrations are some larger than others, but yet all equally swift, (for in a ring of bells the sound of every tone is heard at two or three miles distance in the same order that the bells are struck,) so, I suppose, the ethereal vibrations differ in bigness, but not in swiftness. Now, these vibrations, beside their use in reflection and refraction, may be supposed the chief means by which the parts of fermenting or putrefying substances, fluid liquors, or melted, burning, or other hot bodies, continue in motion." (Birch, Vol. III. p. 251, Dec. 1675.)

"When a ray of light falls upon the surface of any pellucid body, and is there refracted or reflected, may not waves of vibrations, or tremors, be thereby excited in the refracting or reflecting medium? And are not these vibrations propagated from the point of incidence to great distances? And do they not overtake the rays of light, and by overtaking them successively, do not they put them into the fits of easy reflection and easy transmission described above?" (Optics, Qu. 17.)

"Light is in fits of easy reflection and easy transmission, before its incidence on transparent bodies. And probably it is put into such fits at its first emission from luminous bodies, and continues in them during all its progress." (Optics, Second Book, Part III. Prop. 13.)

Hypothesis III.

The Sensation of different Colours depends on the different frequency of Vibrations excited by Light in the Retina.

Passages from Newton.

"The objector's hypothesis, as to the fundamental part of it, is not against me. That fundamental supposition is, that the

parts of bodies, when briskly agitated, do excite vibrations in the ether, which are propagated every way from those bodies in straight lines, and cause a sensation of light by beating and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing. Now, the most free and natural application of this hypothesis to the solution of phenomena I take to be this—that the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the ether of various depths or bignesses, which, being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal bignesses be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours; much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various bignesses, which, according to those bignesses, make several tones in sound: that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction; whence the vibrations of several bignesses, that is, the rays of several colours, which are blended together in light, must be parted from one another by refraction, and so cause the phenomena of prisms and other refracting substances; and that it depends on the thickness of a thin transparent plate or bubble, whether a vibration shall be reflected at its further superficies, or transmitted; so that, according to the number of vibrations, interceding the two superficies, they may be reflected or transmitted for many successive thicknesses. And, since the vibrations which make blue and violet are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate; which is sufficient to explicate all the ordinary phenomena of those plates or bubbles, and also of all natural bodies, whose parts are like so many fragments of such plates. These seem to be the most plain, genuine, and necessary conditions of this hypothesis; and they agree so justly with my theory, that, if the animadversor think fit to

apply them, he need not, on that account, apprehend a divorce from it; but yet, how he will defend it from other difficulties I know not." (Phil. Trans. Vol. VII. p. 5088. Abr. Vol. I. p. 145. Nov. 1672.)

"To explain colours, I suppose, that as bodies of various sizes, densities, or sensations, do by percussion or other action excite sounds of various tones, and consequently vibrations in the air of different bigness; so the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the ether, of various bigness; the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power: and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerves, into the sensorium; and there, I suppose, affect the sense with various colours, according to their bigness and mixture; the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green, and a confusion of all with white—much after the manner that, in the sense of hearing, nature makes use of aerial vibrations of several bignesses to generate sounds of divers tones, for the analogy of nature is to be observed." (Birch, Vol. III. p. 262. Dec. 1675.)

"Considering the lastingness of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the most refrangible rays excite the shortest vibrations, the least refrangible the largest? May not the harmony and discord of colours arise from the proportions of the vibrations propagated through the fibres of the optic nerve into the brain, as the harmony and discord of sounds arise from the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.)

Scholium. Since, for the reason here assigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibra-

tions must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited, for instance, to the three principal colours, red, yellow, and blue; of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion less or more forcibly by undulations differing less or more from a perfect unison; for instance, the undulations of green light being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed of those two species; and each sensitive filament of the nerve may consist of three portions, one for each principal colour. Allowing this statement, it appears that any attempt to produce a musical effect from colours must be unsuccessful, or at least that nothing more than a very simple melody could be imitated by them; for the period, which in fact constitutes the harmony of any concord, being a multiple of the periods of the single undulations, would in this case be wholly without the limits of sympathy of the retina, and would lose its effect, in the same manner as the harmony of a third or fourth is destroyed by depressing it to the lowest notes of the audible scale. In hearing, there seems to be no permanent vibration of any part of the organ.

Hypothesis IV.

All material Bodies have an Attraction for the ethereal Medium, by means of which it is accumulated within their Substance, and for a small Distance around them, in a state of greater Density, but not of greater Elasticity.

It has been shown that the three former hypotheses, which may be called essential, are literally parts of the more complicated Newtonian system. This fourth hypothesis differs perhaps in some degree from any that have been proposed by former authors, and is diametrically opposite to that of Newton; but both being in themselves equally probable, the opposition is

merely accidental, and it is only to be inquired which is the best capable of explaining the phenomena. Other suppositions might perhaps be substituted for this, and therefore I do not consider it as fundamental, yet it appears to be the simplest and best of any that have occurred to me.

Proposition I.

All impulses are propagated in a homogeneous elastic Medium with an equable Velocity.

Every experiment relative to sound coincides with the observation already quoted from Newton, that all undulations are propagated through the air with equal velocity; and this is further confirmed by calculations. (Lagrange. Misc. Taur. Vol. I. p. 91. Also, much more concisely, in my Syllabus of a course of Lectures on Natural and Experimental Philosophy, about to be published. Article 289.) If the impulse be so great as materially to disturb the density of the medium, it will be no longer homogeneous; but, as far as concerns our senses, the quantity of motion may be considered as infinitely small. It is surprising that Euler, although aware of the matter of fact, should still have maintained that the more frequent undulations are more rapidly propagated. (Theor. mus. and Conject. phys.) It is possible that the actual velocity of the particles of the luminiferous ether may bear a much less proportion to the velocity of the undulations than in sound, for light may be excited by the motion of a body moving at the rate of only one mile in the time that light moves a hundred millions.

Scholium 1. It has been demonstrated that in different mediums the velocity varies in the subduplicate ratio of the force directly, and of the density inversely. (Misc. Taur. Vol. I. p. 91. Young's Syllabus. Art. 294.)

Scholium 2. It is obvious, from the phenomena of elastic bodies, and of sounds, that the undulations may cross each other without interruption; but there is no necessity that the various colours of white light should intermix their undulations, for, supposing the vibrations of the retina to continue but a five hundredth of a second after their excitement, a million

undulations of each of a million colours may arrive in distinct succession within this interval of time, and produce the same sensible effect, as if all the colours arrived precisely at the same instant.

Proposition II.

An Undulation conceived to originate from the Vibration of a single Particle, must expand through a homogeneous Medium in a spherical Form, but with different quantities of Motion in different Parts.

For, since every impulse, considered as positive or negative, is propagated with a constant velocity, each part of the undulation must in equal times have passed through equal distances from the vibrating point. And, supposing the vibrating particle, in the course of its motion, to proceed forwards to a small distance in a given direction, the principal strength of the undulation will naturally be straight before it; behind it, the motion will be equal, in a contrary direction; and, at right angles to the line of vibration, the undulation will be evanescent.

Now, in order that such an undulation may continue its progress to any considerable distance, there must be in each part of it a tendency to preserve its own motion in a right line from the centre; for if the excess of force at any part were communicated to the neighbouring particles, there can be no reason why it should not very soon be equalized throughout, or, in other words, become wholly extinct, since the motions in contrary directions would naturally destroy each other. The origin of sound from the vibration of a chord is evidently of this nature; on the contrary, in a circular wave of water, every part is at the same instant either elevated or depressed. It may be difficult to show mathematically the mode in which this inequality of force is preserved, but the inference from the matter of fact appears to be unavoidable; and while the science of hydrodynamics is so imperfect that we cannot even solve the simple problem of the time required to empty a vessel by a given aperture, it cannot be expected that we should be able to account perfectly for so complicated a series of phenomena as

those of elastic fluids. The theory of Huygens, indeed, explains the circumstance in a manner tolerably satisfactory. He supposes every particle of the medium to propagate a distinct undulation in all directions, and that the general effect is only perceptible where a portion of each undulation conspires in direction at the same instant; and it is easy to show that such a general undulation would in all cases proceed rectilinearly, with proportionate force; but, upon this supposition, it seems to follow, that a greater quantity of force must be lost by the divergence of the partial undulations than appears to be consistent with the propagation of the effect to any considerable distance; yet it is obvious that some such limitation of the motion must naturally be expected to take place, for, if the intensity of the motion of any particular part, instead of continuing to be propagated straight forwards, were supposed to affect the intensity of a neighbouring part of the undulation, an impulse must then have travelled from an internal to an external circle in an oblique direction, in the same time as in the direction of the radius, and consequently with a greater velocity, against the first proposition. In the case of water, the velocity is by no means so rigidly limited as in that of an elastic medium. Yet it is not necessary to suppose, nor is it indeed probable, that there is absolutely not the least lateral communication of the force of the undulation, but that, in highly elastic mediums, this communication is almost insensible. In the air, if a chord be perfectly insulated, so as to propagate exactly such vibrations as have been described, they will in fact be much less forcible than if the chord be placed in the neighbourhood of a sounding-board, and probably in some measure because of this lateral communication of motions of an opposite tendency. And the different intensity of different parts of the same circular undulation may be observed, by holding a common tuning-fork at arm's length, while sounding, and turning it, from a plane directed to the ear, into a position perpendicular to that plane.

Proposition III.

A Portion of a spherical Undulation, admitted through an Aperture into a quiescent Medium, will proceed to be further propagated rectilinearly in concentric Superficies, terminated laterally by weak and irregular Portions of newly diverging Undulations.

At the instant of admission, the circumference of each of the undulations may be supposed to generate a partial undulation, filling up the nascent angle between the radii and the surface terminating the medium; but no sensible addition will be made to its strength by a divergence of motion from any other parts of the undulation, for want of a coincidence in time, as has already been explained with respect to the various force of a spherical undulation. If indeed the aperture bear but a small proportion to the breadth of an undulation, the newly generated undulation may nearly absorb the whole force of the portion admitted; and this is the case considered by Newton in the Principia. But no experiment can be made under these circumstances with light, on account of the minuteness of its undulations, and the interference of inflection; and yet some faint radiations do actually diverge beyond any probable limits of inflection, rendering the margin of the aperture distinctly visible in all directions. These are attributed by Newton to some unknown cause, distinct from inflection (Optics, Third Book, Obs. 5); and they fully answer the description of this proposition.

Let the concentric lines in Fig. 128 represent the contemporaneous situation of similar parts of a number of successive undulations diverging from the point A; they will also represent the successive situations of each individual undulation: let the force of each undulation be represented by the breadth of the line, and let the cone of light ABC be admitted through the aperture BC; then the principal undulations will proceed in a rectilinear direction towards GH, and the faint radiations on each side will diverge from B and C as centres, without receiving any additional force from any intermediate point D

of the undulation, on account of the inequality of the lines DE and DF. But if we allow some little lateral divergence from the extremities of the undulations, it must diminish their force, without adding materially to that of the dissipated light; and their termination, instead of the right line BG, will assume the form CH, since the loss of force must be more considerable near to C than at greater distances. This line corresponds with the boundary of the shadow in Newton's first observation, Fig. 128; and it is much more probable that such a dissipation of light was the cause of the increase of the shadow in that observation, than that it was owing to the action of the inflecting atmosphere, which must have extended a thirtieth of an inch each way in order to produce it; especially when it is considered that the shadow was not diminished by surrounding the hair with a denser medium than air, which must in all probability have weakened and contracted its inflecting atmosphere. In other circumstances the lateral divergence might appear to increase, instead of diminishing, the breadth of the beam.

As the subject of this proposition has always been esteemed the most difficult part of the undulatory system, it will be proper to examine here the objections which Newton has grounded upon it.

"To me the fundamental supposition itself seems impossible, namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in straight lines, without a continual and very extravagant spreading and bending every way into the quiescent medium, where they are terminated by it. I mistake if there be not both experiment and demonstration to the contrary." (Phil. Trans. VII. 5089. Abr. I. 146. Nov. 1672.)

"Motus omnis per fluidum propagatus divergit a recto tramite in spatia immota."

"Quoniam medium ibi," in the middle of an undulation admitted, "densius est, quam in spatiis hinc inde, dilatabit sese tam versus spatia utrinque sita, quam versus pulsuum rariora intervalla; eoque pacto—pulsus eadem *fere* celeritate sese in medii partes quiescentes hinc inde relaxare debent;—ideoque

spatium totum occupabunt.—Hoc experimur in sonis." (Princip. Lib. II. Prop. 42.)

"Are not all hypotheses erroneous, in which light is supposed to consist in pression or motion, propagated through a fluid medium?—If it consisted in pression or motion, propagated either in an instant, or in time, it would bend into the shadow. For pression or motion cannot be propagated in a fluid in right lines beyond an obstacle which stops part of the motion, but will bend and spread every way into the quiescent medium which lies beyond the obstacle. The waves on the surface of stagnating water, passing by the sides of a broad obstacle which stops part of them, bend afterwards, and dilate themselves gradually into the quiet water behind the obstacle. The waves, pulses, or vibrations of the air, wherein sounds consist, bend manifestly, though not so much as the waves of water. For a bell or a cannon may be heard beyond a hill, which intercepts the sight of the sounding body; and sounds are propagated as readily through crooked pipes as straight ones. But light is never known to follow crooked passages nor to bend into the shadow. For the fixed stars, by the interposition of any of the planets, cease to be seen. And so do the parts of the sun, by the interposition of the moon, Mercury, or Venus. The rays which pass very near to the edges of any body, are bent a little by the action of the body; but this bending is not towards but from the shadow, and is performed only in the passage of the ray by the body, and at a very small distance from it. So soon as the ray is past the body, it goes right on." (Optics, Qu. 28.)

Now the proposition quoted from the Principia does not directly contradict this proposition; for it does not assert that such a motion must diverge equally in all directions; neither can it with truth be maintained, that the parts of an elastic medium communicating any motion, must propagate that motion equally in all directions.* All that can be inferred by reasoning is, that the marginal parts of the undulation must be somewhat weakened, and that there must be a faint divergence in every direction; but whether either of these effects might be of sufficient magnitude to be sensible, could not have been inferred

* Supra, p. 66–69.

from argument, if the affirmative had not been rendered probable by experiment.

As to the analogy with other fluids, the most natural inference from it is this: "The waves of the air, wherein sounds consist, bend manifestly, though not so much as the waves of water;" water being an inelastic, and air a moderately elastic medium; but ether being most highly elastic, its waves bend very far less than those of the air, and therefore almost imperceptibly. Sounds are propagated through crooked passages, because their sides are capable of reflecting sound, just as light would be propagated through a bent tube, if perfectly polished within.

The light of a star is by far too weak to produce, by its faint divergence, any visible illumination of the margin of a planet eclipsing it; and the interception of the sun's light by the moon, is as foreign to the question, as the statement of inflection is inaccurate.

To the argument adduced by Huygens, in favour of the rectilinear propagation of undulations, Newton has made no reply; perhaps because of his own misconception of the nature of the motions of elastic mediums, as dependent on a peculiar law of vibration, which has been corrected by later mathematicians.* On the whole, it is presumed, that this proposition may be safely admitted as perfectly consistent with analogy and with experiment.

Proposition IV.

When an Undulation arrives at a Surface which is the Limit of Mediums of different Densities, a partial Reflection takes place, proportionate in Force to the Difference of the Densities.

This may be illustrated, if not demonstrated, by the analogy of elastic bodies of different sizes. "If a smaller elastic body strikes against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one propels it,

* Supra, p. 72.

without losing all its motion: thus, the particles of a denser stratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum."* But it is not absolutely necessary to suppose an attraction in the latter case, since the effort to proceed would be propagated backwards without it, and the undulation would be reversed, a rarefaction returning in place of a condensation; and this will perhaps be found most consistent with the phenomena.

Proposition V.

When an Undulation is transmitted through a surface terminating different Mediums, it proceeds in such a direction, that the Sines of the Angles of Incidence and Refraction are in the constant ratio of the Velocity of Propagation in the two Mediums.

(Barrow, Lect. Opt. II. p. 4. Huygens, *de la Lum.* cap. 3. Euler, *Conj. Phys.* Young's Syllabus. Art. 382.)†

Corollary 1. The same demonstrations prove the equality of the angles of reflection and incidence.

Corollary 2. It appears from experiments on the refraction of condensed air, that the ratio of the difference of the sines varies simply as the density. Hence it follows, by Schol. I. Prop. I. that the excess of the density of the ethereal medium is in the duplicate ratio of the density of the air; each particle co-operating with its neighbours in attracting a greater portion of it.

Proposition VI.

When an Undulation falls on the Surface of a rarer Medium, so obliquely that it cannot be regularly refracted, it is totally reflected, at an Angle equal to that of its Incidence.‡

Corollary. This phenomenon tends to prove the gradual increase and diminution of density at the surface terminating two

* Supra, p. 80. † Supra, p. 81. ‡ Ibid.

mediums, as supposed in Hypothesis 4; although Huygens has attempted to explain it somewhat differently.

PROPOSITION VII.

If equidistant Undulations be supposed to pass through a Medium, of which the Parts are susceptible of permanent Vibrations somewhat slower than the Undulations, their velocity will be somewhat lessened by this vibratory Tendency; and, in the same Medium, the more, as the Undulations are more frequent.

For, as often as the state of the undulation requires a change in the actual motion of the particle which transmits it, that change will be retarded by the propensity of the particle to continue its motion somewhat longer: and this retardation will be more frequent and more considerable, as the difference between the periods of the undulation and of the natural vibration is greater.

Corollary. It was long an established opinion, that heat consists in vibrations of the particles of bodies, and is capable of being transmitted by undulations through an apparent vacuum. (Newt. Opt. Qu. 18.) This opinion has been of late very much abandoned. Count Rumford, Professor Pictet, and Mr. Davy, are almost the only authors who have appeared to favour it; but it seems to have been rejected without any good grounds, and will probably very soon recover its popularity.

Let us suppose that these vibrations are less frequent than those of light; all bodies therefore are liable to permanent vibrations slower than those of light; and indeed almost all are liable to luminous vibrations, either when in a state of ignition, or in the circumstances of solar phosphori; but much less easily, and in a much less degree, than to the vibrations of heat. It will follow from these suppositions, that the more frequent luminous undulations will be more retarded than the less frequent; and consequently, that blue light will be more refrangible than red, and radiant heat least of all; a consequence which coincides exactly with the highly interesting experiments of Dr. Herschel. (Phil. Trans. for 1800, p. 284.) It may also be easily conceived, that the actual existence of a state of slower vibration may tend still more to retard the more fre-

quent undulations, and that the refractive power of solid bodies may be sensibly increased by an increase of temperature, as it actually appears to have been in Euler's experiments. (Acad. de Berlin, 1762, p. 328.)

Scholium. If, notwithstanding, this proposition should appear to be insufficiently demonstrated, it must be allowed to be at least equally explanatory of the phenomena with anything that can be advanced on the other side, from the doctrine of projectiles; since a supposed accelerating force must act in some other proportion than that of the bulk of the particles; and, if we call this an elective attraction, it is only veiling under a chemical term, our incapacity of assigning a mechanical cause. Mr. Short, when he found by observation the equality of the velocity of light of all colours, felt the objection so forcibly, that he immediately drew an inference from it in favour of the undulatory system. It is assumed in the proposition, that when light is dispersed by refraction, the corpuscles of the refracting substance are in a state of actual alternate motion, and contribute to its transmission; but it must be confessed that we cannot at present form a very decided and accurate conception of the forces concerned in maintaining these corpuscular vibrations.

Proposition VIII.

When two Undulations, from different Origins, coincide either perfectly or very nearly in Direction, their joint effect is a Combination of the Motions belonging to each.

Since every particle of the medium is affected by each undulation, wherever the directions coincide, the undulations can proceed no otherwise than by uniting their motions, so that the joint motion may be the sum or difference of the separate motions, accordingly as similar or dissimilar parts of the undulations are coincident.

I have, on a former occasion, insisted at large on the application of this principle to harmonics;* and it will appear to be of still more extensive utility in explaining the phenomena of colours. The undulations which are now to be compared are those of equal frequency. When the two series coincide exactly

* Supra, p. 83.

in point of time, it is obvious that the united velocity of the particular motions must be greatest, and, in effect at least, double the separate velocities; and also, that it must be smallest, and if the undulations are of equal strength, totally destroyed, when the time of the greatest direct motion belonging to one undulation coincides with that of the greatest retrograde motion of the other. In intermediate states, the joint undulation will be of intermediate strength; but by what laws this intermediate strength must vary, cannot be determined without further data. It is well known that a similar cause produces in sound, that effect which is called a beat; two series of undulations of nearly equal magnitude co-operating and destroying each other alternately, as they coincide more or less perfectly in the times of performing their respective motions.

COROLLARY I.—*Of the Colours of striated Surfaces.*

Boyle appears to have been the first that observed the colours of scratches on polished surfaces. Newton has not noticed them. Mazeas and Mr. Brougham* have made some experiments on the subject, yet without deriving any satisfactory conclusion. But all the varieties of these colours are very easily deduced from this proposition.

Let there be in a given plane two reflecting points very near each other, and let the plane be so situated that the reflected image of a luminous object seen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with respect to both points, considering them as capable of reflecting in all directions. Let one of the points be now depressed below the given plane; then the whole part of the light reflected from it, will be lengthened by a line which is to the depression of the point as twice the cosine of incidence to the radius. Fig. 129.

If, therefore, equal undulations of given dimensions be reflected from two points, situated near enough to appear to the eye but as one, wherever this line is equal to half the breadth of a whole undulation, the reflection from the depressed point will so interfere with the reflection from the fixed point, that the

* Phil. Trans. for 1797, vol. lxxxvii. p. 352.

progressive motion of the one will coincide with the retrograde motion of the other, and they will both be destroyed; but, when this line is equal to the whole breadth of an undulation, the effect will be doubled; and when to a breadth and a half, again destroyed; and thus for a considerable number of alternations: and, if the reflected undulations be of different kinds, they will be variously affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of retardation.

In order that the effect may be the more perceptible, a number of pairs of points must be united into two parallel lines; and, if several such pairs of lines be placed near each other, they will facilitate the observation. If one of the lines be made to revolve round the other as an axis, the depression below the given plane will be as the sine of the inclination; and, while the eye and luminous object remain fixed, the difference of the length of the paths will vary as this sine.

The best subjects for the experiment are Mr. Coventry's exquisite micrometers; such of them as consist of parallel lines drawn on glass, at the distance of one five hundredth of an inch, are the most convenient. Each of these lines appears under a microscope to consist of two or more finer lines, exactly parallel, and at the distance of somewhat more than a twentieth of that of the adjacent lines. I placed one of these so as to reflect the sun's light at an angle of 45°, and fixed it in such a manner, that while it revolved round one of the lines as an axis, I could measure its angular motion; and I found that the brightest red colour occurred at the inclinations $10\frac{1}{4}$, $20\frac{3}{4}$ °, 32°, and 45°; of which the sines are as the numbers 1, 2, 3, and 4. At all other angles also, when the sun's light was reflected from the surface, the colour vanished with the inclination, and was equal at equal inclinations on either side.

This experiment affords a very strong confirmation of the theory. It is impossible to deduce any explanation of it from any hypothesis hitherto advanced; and I believe it would be difficult to invent any other that would account for it. There is a striking analogy between this separation of colours, and the production of a musical note by successive echoes from equi-

distant iron palisades; which I have found to correspond pretty accurately with the known velocity of sound, and the distances of the surfaces.

It is not improbable that the colours of the integuments of some insects, and of some other natural bodies, exhibiting in different lights the most beautiful versatility, may be found to be of this description, and not to be derived from thin plates. In some cases, a single scratch or furrow may produce similar effects, by the reflection of its opposite edges.

Corollary II.—*Of the Colours of thin Plates.*

When a beam of light falls on two parallel refracting surfaces, the partial reflections coincide perfectly in direction; and, in this case, the interval of retardation, taken between the surfaces, is to their distance as twice the cosine of the angle of refraction to the radius. For, in Fig. 130, drawing AB and CD perpendicular to the rays, the times of passing through BC and AD will be equal, and DE will be half the interval of retardation; but DE is to CE as the sine of DCE to the radius. Hence, that DE may be constant, or that the same colour may be reflected, the thickness CE must vary as the secant of the angle of refraction CED; which agrees exactly with Newton's experiments; for the correction is perfectly inconsiderable.

Let the medium between the surfaces be rarer than the surrounding mediums; then the impulse reflected at the second surface, meeting a subsequent undulation at the first, will render the particles of the rarer medium capable of wholly stopping the motion of the denser, and destroying the reflection (Prop. iv.), while they themselves will be more strongly propelled than if they had been at rest; and the transmitted light will be increased. So that the colours by reflection will be destroyed, and those by transmission rendered more vivid, when the double thicknesses, or intervals of retardation, are any multiples of the whole breadths of the undulations; and, at intermediate thicknesses, the effects will be reversed; according to the Newtonian observations.

If the same proportions be found to hold good with respect to thin plates of a denser medium, which is indeed not improbable, it will be necessary to adopt the corrected demonstration

of Prop. iv., but, at any rate, if a thin plate be interposed between a rarer and a denser medium, the colours by reflection and transmission may be expected to change places.

From Newton's measures of the thicknesses reflecting the different colours, the breadth and duration of their respective undulations may be very accurately determined; although it is not improbable, that when the glasses approach very near, the atmosphere of ether may produce some little irregularity. The whole visible spectrum appears to be comprised within the ratio of three to five, or a major sixth in music; and the undulations of red, yellow, and blue, to be related in magnitude as the numbers 7, 8, and 6; so that the interval from red to blue* is a fourth. The absolute frequency expressed in numbers is too great to be distinctly conceived, but it may be better imagined by a comparison with sound. If a chord sounding the tenor $\bar{c}$, could be continually bisected 40 times, and should then vibrate, it would afford a yellow green light: this being denoted by $\bar{c}^{41}$, the extreme red would be $\bar{a}^{40}$, and the blue $\bar{d}^{41}$. The absolute length and frequency of each vibration is expressed in the table; supposing light to travel in $8\frac{1}{6}$ minutes 500,000,000,000 feet.

Colours.	Length of an Undulation in parts of an Inch, in Air.	Number of Undulations in an Inch.	Number of Undulations in a Second.
Extreme . .	.0000266	37640	463 millions of millions.
Red	.0000256	39180	482
Intermediate .	.0000246	40720	501
Orange	.0000240	41610	512
Intermediate. .	.0000235	42510	523
Yellow	.0000227	44000	542
Intermediate .	.0000219	45600	561 (= 2^{48} nearly)
Green	.0000211	47460	584
Intermediate .	.0000203	49320	607
Blue	.0000196	51110	629
Intermediate .	.0000189	52910	652
Indigo	.0000185	54070	665
Intermediate .	.0000181	55240	680
Violet	.0000174	57490	707
Extreme . . .	.0000167	59750	735

* See the correction given in No. VIII., p. 177: for "red, yellow, and blue" substitute "red, green, and violet;" and for the numbers "8, 7, and 6" substitute "7, 6, and 5."—*Note by the Editor.*

Scholium. It was not till I had satisfied myself respecting all these phenomena. that I found in Hooke's Micrographia, a passage which might have led me earlier to a similar conclusion. "It is most evident that the reflection from the under or further side of the body, is the principal cause of the production of these colours.—Let the ray fall obliquely on the thin plate, part therefore is reflected back by the first superficies,—part refracted to the second surface,—whence it is reflected and refracted again. So that, after two refractions and one reflection, there is propagated a kind of fainter ray,—" and, "by reason of the time spent in passing and repassing,—this fainter pulse comes behind the" former reflected "pulse; so that hereby, (the surfaces being so near together that the eye cannot discriminate them from one,) this confused or duplicated pulse, whose strongest part precedes, and whose weakest follows, does produce on the retina the sensation of a yellow. If these surfaces are further removed as under, the weaker pulse may become coincident with the" reflection of the "second," or next following pulse, from the first surface, "and lag behind that also, and be coincident with the third, fourth, fifth, sixth, seventh, or eighth—; so that if there be a thin transparent body, that from the greatest thinness requisite to produce colours, does by degrees grow to the greatest thickness,—the colours shall be so often repeated, as the weaker pulse does lose paces with its primary or first pulse, and is coincident with a" subsequent "pulse. And this, as it is coincident, or follows from the first hypothesis I took of colours, so upon experiment have I found it in multitudes of instances that seem to prove it." (P. 65—67.) This was printed about seven years before any of Newton's experiments were made. We are informed by Newton, that Hooke was afterwards disposed to adopt his "suggestion" of the nature of colours; and yet it does not appear that Hooke ever applied that improvement to his explanation of these phenomena, or inquired into the necessary consequence of a change of obliquity, upon his original supposition, otherwise he could not but have discovered a striking coincidence with the measures laid down by Newton from experiment. All former attempts to explain the colours

of thin plates, have either proceeded on suppositions, which, like Newton's, would lead us to expect the greatest irregularities in the direction of the refracted rays; or, like Mr. Michell's, would require such effects from the change of the angle of incidence, as are contrary to the effects observed; or, they are equally deficient with respect to both these circumstances, and are inconsistent with the most moderate attention to the principal phenomena.

Corollary III.—*Of the Colours of thick Plates.*

When a beam of light passes through a refracting surface, especially if imperfectly polished, a portion of it is irregularly scattered, and makes the surface visible in all directions, but most conspicuously in directions not far distant from that of the light itself; and, if a reflecting surface be placed parallel to the refracting surface, this scattered light, as well as the principal beam, will be reflected, and there will also be a new dissipation of light, at the return of the beam through the refracting surface. These two portions of scattered light will coincide in direction; and if the surfaces be of such a form as to collect the similar effects, will exhibit rings of colours. The interval of retardation is here, the difference between the paths of the principal beam and of the scattered light between the two surfaces: of course, wherever the inclination of the scattered light is equal to that of the beam, although in different planes, the interval will vanish, and all the undulations will conspire. At other inclinations, the interval will be the difference of the secants from the secant of the inclination or angle of refraction of the principal beam. From these causes, all the colours of concave mirrors observed by Newton and others are necessary consequences: and it appears that their production, though somewhat similar, is by no means, as Newton imagined, identical with the production of those of thin plates.

Corollary IV.—*Of Blackness.*

In the three preceding corollaries, we have considered the refracting and reflecting substances as limited by a mathema-

tical surface: but this is perhaps never physically true. The ethereal atmospheres may extend on each side the surface as far as the breadth of one or more undulations; and, if they be supposed to vary equally in density at every part, the partial reflections from each of the infinite number of surfaces, where the density changes, will very much interfere with each other, and destroy a considerable portion of the reflected light, so that the substance may become positively black; and this effect may take place in a greater or less degree, as the density of the ethereal atmosphere varies more or less equably; and, in some cases, particular undulations being more affected than others, a tinge of colour may be produced. Accordingly, M. Bouguer has observed a considerable loss of light, and in some instances a tinge of colour, in total reflections at the surface of a rarer medium.

Corollary V.—*Of Colours by Inflection.*

Whatever may be the cause of the inflection of light passing through a small aperture, the light nearest its centre must be the least diverted, and the nearest to its sides the most: another portion of light falling very obliquely on the margin of the aperture, will be copiously reflected in various directions; some of which will either perfectly or very nearly coincide in direction with the unreflected light, and, having taken a circuitous route, will so interfere with it, as to cause an appearance of colours. The length of the two tracks will differ the less, as the direction of the reflected light has been less changed by its reflection, that is, in the light passing nearest to the margin; so that the blues will appear in the light nearest the shadow. The effect will be increased and modified when the reflected light falls within the influence of the opposite edge, so as to interfere with the light simply inflected by that also.

But in order to examine the consequences more minutely, it will be convenient to suppose the inflection caused by an ethereal atmosphere, of a density varying as a given power of the distance from a centre, as in the eighth proposition of the last Bakerian Lecture.* Putting $r = 3$, and $x = \frac{1}{2}$, I have constructed

* Supra, p. 20.

a diagram, (Fig. 131,) which shows by the two pairs of curves the relative position of the reflected and unreflected portions of any one undulation at two successive times, and also, by shaded lines drawn across the parts where the intervals of retardation are in arithmetical progression, and where similar colours will be exhibited at different distances from the inflecting substance. The result fully agrees with the observations of Newton's third book, and with those of later writers. But I do not consider it as quite certain, until further experiments have been made on the inflecting power of different substances, that Dr. Hooke's explanation of inflection, by the tendency of light to diverge, may not have some pretensions to truth. I am sorry to be obliged to recall here the assent which, at first sight, I was induced to give to a supposed improvement of a late author.*

Scholium. In the construction of the diagram, it becomes necessary to find the time spent by each ray in its passage. Since the velocity was denoted by $x^{-\frac{1}{r}}$, on the supposition of a projectile, it will be as $x^{\frac{1}{r}}$, on the contrary supposition† (*Schol.* 2. Prop. I.), and the fluxion of the distance described being $\frac{\dot{x}}{\sqrt{1-yy}}$, that of the time will be $\frac{x^{-\frac{1}{r}}\dot{x}}{\sqrt{1-yy}}$ or $\frac{rs}{1-r}\cdot\frac{\dot{y}}{yy\cdot\sqrt{1-yy}}$ of which the fluent is $\frac{r}{1-r}\cdot\frac{s}{y}\cdot\sqrt{1-yy}$. Therefore, with the radius $x^{1-\frac{1}{r}}$, describe a circle concentric with the surfaces of the inflecting atmosphere, then the angle described by the ray during its passage through the atmosphere, will always be to the angle subtended by the line cut off by this circle from the incident ray produced, in the ratio of r to $r-1$; and the time spent in this passage will be in the same ratio to the time that would have been spent in describing this intercepted portion with the initial velocity. For y, being equal to $s\,x^{\frac{1}{r}-1}$, is the sine of the inclination of the incident ray to the radius, where it meets this circle; therefore, by the

* Supra, p. 81. † Supra, p. 15.

proposition quoted, the angle described is in a given ratio to the angle at the centre, which is the difference of the inclinations. Making $x^{-\frac{1}{r}}$ or $\frac{s}{y}$ radius, the sine, instead of y, becomes s, and the cosine $\sqrt{\frac{ss}{yy} - ss}$, or $\frac{s}{y}\sqrt{1-yy}$, and, when $y = ss$, $\sqrt{1-ss}$; therefore the line intercepted is to the difference of the fluents as r to $r-1$. (See also Young's Syllabus, Art. 372.)

Proposition IX.

Radiant Light consists in undulations of the luminiferous Ether.

This proposition is the general conclusion from all the preceding, and it is conceived that they conspire to prove it in as satisfactory a manner as can possibly be expected from the nature of the subject. It is clearly granted by Newton, that there are undulations, yet he denies that they constitute light; but it is shown in the three first corollaries of the last proposition, that all cases of the increase or diminution of light are referable to an increase or diminution of such undulations, and that all the affections to which the undulations would be liable, are distinctly visible in the phenomena of light; it may therefore be very logically inferred, that the undulations are light.

A few detached remarks will serve to obviate some objections which may be raised against this theory.

1. Newton has advanced the singular refraction of the Iceland crystal, as an argument that the particles of light must be projected corpuscles; since he thinks it probable that the different sides of these particles must be differently attracted by the crystal, and since Huygens has confessed his inability to account in a satisfactory manner for all the phenomena. But, contrarily to what might have been expected from Newton's usual accuracy and candour, he has laid down a new law for the refraction, without giving a reason for rejecting that of Huygens, which Mr. Haüy has found to be more accurate than Newton's; and, without attempting to deduce from his own system any explanation of the more universal and striking

effects of doubling spars, he has omitted to observe that Huygens's most elegant and ingenious theory perfectly accords with these general effects, in all particulars, and of course derives from them additional pretensions to truth; this he omits, in order to point out a difficulty, for which only a verbal solution can be found in his own theory, and which will probably long remain unexplained by any other.

2. Mr. Michell has made some experiments, which appear to show that the rays of light have an actual momentum, by means of which a motion is produced when they fall on a thin plate of copper delicately suspended. (Priestley's Optics.) But, taking for granted the exact perpendicularity of the plate, and the absence of any ascending current of air, yet since, in every such experiment, a greater quantity of heat must be communicated to the air at the surface on which the light falls than at the opposite surface, the excess of expansion must necessarily produce an excess of pressure on the first surface, and a very perceptible recession of the plate in the direction of the light. Mr. Bennet has repeated the experiment, with a much more sensible apparatus, and also in the absence of air; and very justly infers from its total failure, an argument in favour of the undulatory system of light. (Phil. Trans. for 1792, p. 87.) For, granting the utmost imaginable subtility of the corpuscles of light, their effects might naturally be expected to bear some proportion to the effects of the much less rapid motions of the electrical fluid, which are so very easily perceptible, even in their weakest states.

3. There are some phenomena of the light of solar phosphori, which at first sight might seem to favour the corpuscular system; for instance, its remaining many months as if in a latent state, and its subsequent re-emission by the action of heat. But, on further consideration, there is no difficulty in supposing the particles of the phosphori which have been made to vibrate by the action of light, to have this action abruptly suspended by the intervention of cold, whether as contracting the bulk of the substance or otherwise; and again, after the restraint is removed, to proceed in their motion, as a spring would do which had been held fast for a time in an intermediate stage of

its vibration; nor is it impossible that heat itself may, in some circumstances, become in a similar manner latent. (Nicholson's Journal, vol. ii. p. 399.) But the affections of heat may perhaps hereafter be rendered more intelligible to us; at present, it seems highly probable that light differs from heat only in the frequency of its undulations or vibrations; those undulations which are within certain limits, with respect to frequency, being capable of affecting the optic nerve, and constituting light; and those which are slower and probably stronger, constituting heat only; that light and heat occur to us, each in two predicaments, the vibratory or permanent, and the undulatory or transient state; vibratory light being the minute motion of ignited bodies, or of solar phosphori, and undulatory or radiant light the motion of the ethereal medium excited by these vibrations; vibratory heat being a motion to which all material substances are liable, and which is more or less permanent; and undulatory heat that motion of the same ethereal medium, which has been shown by Mr. King (Morsels of Criticism, 1786, p. 99), and Mr. Pictet (*Essais de Physique*, 1790), to be as capable of reflection as light, and by Dr. Herschel to be capable of separate refraction. (Phil. Trans. for 1800, p. 284.) How much more readily heat is communicated by the free access of colder substances, than either by radiation or by transmission through a quiescent medium, has been shown by the valuable experiments of Count Rumford. It is easy to conceive that some substances permeable to light, may be unfit for the transmission of heat, in the same manner as particular substances may transmit some kinds of light, while they are opaque with respect to others.

On the whole, it appears, that the few optical phenomena which admit of explanation by the corpuscular system, are equally consistent with this theory; that many others, which have long been known, but never understood, become by these means perfectly intelligible; and that several new facts are found to be thus only reducible to a perfect analogy with other facts, and to the simple principles of the undulatory system. It is presumed, that henceforth the second and third books of Newton's Optics will be considered as more fully understood

Fig. 129.

Fig. 130.

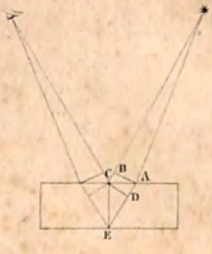


Fig. 128.

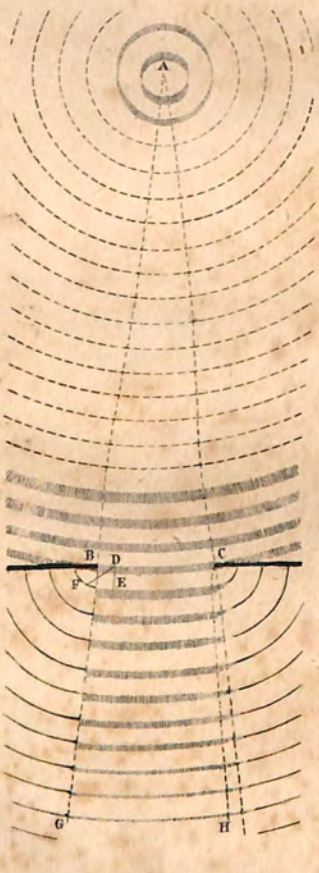


Fig. 131.

Standidge & Co. Lithe. London.

than the first has hitherto been; but, if it should appear to impartial judges, that additional evidence is wanting for the establishment of the theory, it will be easy to enter more minutely into the details of various experiments, and to show the insuperable difficulties attending the Newtonian doctrines, which, without necessity, it would be tedious and invidious to enumerate. The merits of their author in natural philosophy are great beyond all contest or comparison: his optical discovery of the composition of white light would alone have immortalized his name; and the very arguments which tend to overthrow his system, give the strongest proofs of the admirable accuracy of his experiments.

Sufficient and decisive as these arguments appear, it cannot be superfluous to seek for further confirmation; which may with considerable confidence be expected, from an experiment very ingeniously suggested by Professor Robison, on the refraction of the light returning to us from the opposite margins of Saturn's ring: for, on the corpuscular theory, the ring must be considerably distorted when viewed through an achromatic prism: a similar distortion ought also to be observed in the disc of Jupiter; but, if it be found that an equal deviation is produced in the whole light reflected from these planets, there can scarcely be any remaining hope to explain the affections of light by a comparison with the motions of projectiles.

No. VIII.

AN ACCOUNT OF SOME CASES OF THE

PRODUCTION OF COLOURS

NOT HITHERTO DESCRIBED.

From the Philosophical Transactions for 1802, p. 387.

Read July 1, 1802.

Whatever opinion may be entertained of the theory of light and colours which I have lately had the honour of submitting to the Royal Society, it must at any rate be allowed that it has given birth to the discovery of a simple and general law, capable of explaining a number of the phenomena of coloured light, which, without this law, would remain insulated and unintelligible. The law is, that "wherever two portions of the same light arrive at the eye by different routes, either exactly or very nearly in the same direction, the light becomes most intense when the difference of the routes is any multiple of a certain length, and least intense in the intermediate state of the interfering portions; and this length is different for light of different colours."

I have already shown in detail, the sufficiency of this law for explaining all the phenomena described in the second and third books of Newton's Optics, as well as some others not mentioned by Newton. But it is still more satisfactory to observe its conformity to other facts, which constitute new and distinct classes of phenomena, and which could scarcely have agreed so well with any anterior law, if that law had been erroneous or imaginary: these are the colours of fibres, and the colours of mixed plates.

As I was observing the appearance of the fine parallel lines of light which are seen upon the margin of an object held near

the eye, so as to intercept the greater part of the light of a distant luminous object, and which are produced by the fringes caused by the inflection of light already known, I observed that they were sometimes accompanied by coloured fringes, much broader and more distinct; and I soon found that these broader fringes were occasioned by the accidental interposition of a hair. In order to make them more distinct, I employed a horse-hair, but they were then no longer visible. With a fibre of wool, on the contrary, they became very large and conspicuous; and, with a single silk-worm's thread, their magnitude was so much increased, that two or three of them seemed to occupy the whole field of view. They appeared to extend on each side of the candle, in the same order as the colours of thin plates, seen by transmitted light. It occurred to me that their cause must be sought in the interference of two portions of light, one reflected from the fibre, the other bending round its opposite side, and at last coinciding nearly in direction with the former portion; that, accordingly as both portions deviated more from a rectilinear direction, the difference of the length of their paths would become gradually greater and greater, and would consequently produce the appearances of colour usual in such cases; that supposing them to be inflected at right angles, the difference would amount nearly to the diameter of the fibre, and, that this difference must consequently be smaller as the fibre became smaller; and, the number of fringes in a right angle becoming smaller, that their angular distances would consequently become greater, and the whole appearance would be dilated. It was easy to calculate, that for the light least inflected, the difference of the paths would be to the diameter of the fibre, very nearly as the deviation of the ray, at any point, from the rectilinear direction, to its distance from the fibre.

I therefore made a rectangular hole in a card, and bent its ends so as to support a hair parallel to the sides of the hole; then, upon applying the eye near the hole, the hair of course appeared dilated by indistinct vision into a surface, of which the breadth was determined by the distance of the hair and the magnitude of the hole, independently of the temporary aperture

of the pupil. When the hair approached so near to the direction of the margin of a candle that the inflected light was sufficiently copious to produce a sensible effect, the fringes began to appear; and it was easy to estimate the proportion of their breadth to the apparent breadth of the hair, across the image of which they extended. I found that six of the brightest red fringes, nearly at equal distances, occupied the whole of that image. The breadth of the aperture was $\frac{6}{100}$, and its distance from the hair $\frac{8}{10}$ of an inch: the diameter of the hair was less than $\frac{1}{500}$ of an inch; as nearly as I could ascertain, it was $\frac{1}{600}$. Hence, we have $\frac{1}{1000}$ for the deviation of the first red fringe at the distance $\frac{8}{10}$; and, as $\frac{8}{10}$: $\frac{1}{1000}$ :: $\frac{1}{800}$: $\frac{1}{8000}$, or $\frac{1}{4800}$ for the difference of the routes of the red light where it was most intense. The measure deduced from Newton's experiments is $\frac{1}{5000}$. I thought this coincidence, with only an error of one-ninth of so minute a quantity, sufficiently perfect to warrant completely the explanation of the phenomenon, and even to render a repetition of the experiment unnecessary: for there are several circumstances which make it difficult to calculate much more precisely what ought to be the result of the measurement.

When a number of fibres of the same kind, for instance, a uniform lock of wool, are held near to the eye, we see an appearance of halos surrounding a distant candle; but their brilliancy, and even their existence, depends on the uniformity of the dimensions of the fibres; and they are larger as the fibres are smaller. It is obvious that they are the immediate consequences of the coincidence of a number of fringes of the same size, which, as the fibres are arranged in all imaginable directions, must necessarily surround the luminous object at equal distances on all sides, and constitute circular fringes.

There can be little doubt that the coloured atmospherical halos are of the same kind: their appearance must depend on the existence of a number of particles of water, of equal dimensions, and in a proper position, with respect to the luminary and to the eye. As there is no natural limit to the magnitude of the spherules of water, we may expect these halos to vary without limit in their diameters; and accordingly, Mr. Jordan has

observed that their dimensions are exceedingly various, and has remarked that they frequently change during the time of observation.

I first noticed the colours of mixed plates, in looking at a candle through two pieces of plate-glass, with a little moisture between them. I observed an appearance of fringes resembling the common colours of thin plates; and, upon looking for the fringes by reflection, I found that these new fringes were always in the same direction as the other fringes, but many times larger. By examining the glasses with a magnifier, I perceived that wherever these fringes were visible, the moisture was intermixed with portions of air, producing an appearance similar to dew. I then supposed that the origin of the colours was the same as that of the colours of halos; but, on a more minute examination, I found that the magnitude of the portions of air and water was by no means uniform, and that the explanation was therefore inadmissible. It was, however, easy to find two portions of light sufficient for the production of these fringes; for, the light transmitted through the water, moving in it with a velocity different from that of the light passing through the interstices filled only with air, the two portions would interfere with each other, and produce effects of colour according to the general law. The ratio of the velocities in water and in air is that of 3 to 4; the fringes ought therefore to appear where the thickness is 6 times as great as that which corresponds to the same colour in the common case of thin plates; and, upon making the experiment with a plane glass and a lens slightly convex, I found the sixth dark circle actually of the same diameter as the first in the new fringes. The colours are also very easily produced, when butter or tallow is substituted for water; and the rings then become smaller, on account of the greater refractive density of the oils: but, when water is added, so as to fill up the interstices of the oil, the rings are very much enlarged; for here the difference only of the velocities in water and in oil is to be considered, and this is much smaller than the difference between air and water. All these circumstances are sufficient to satisfy us with respect to the truth of the explanation; and it is still more confirmed by the effect of inclining the

plates to the direction of the light; for then, instead of dilating, like the colours of thin plates, these rings contract: and this is the obvious consequence of an increase of the length of the paths of the light, which now traverses both mediums obliquely; and the effect is every where the same as that of a thicker plate.

It must, however, be observed, that the colours are not produced in the whole light that is transmitted through the mediums: a small portion only of each pencil, passing through the water contiguous to the edges of the particle, is sufficiently coincident with the light transmitted by the neighbouring portions of air, to produce the necessary interference; and it is easy to show that, on account of the natural concavity of the surface of each portion of the fluid adhering to the two pieces of glass, a considerable portion of the light which is beginning to pass through the water will be dissipated laterally by reflection at its entrance, and that much of the light passing through the air will be scattered by refraction at the second surface. For these reasons, the fringes are seen when the plates are not directly interposed between the eye and the luminous object; and on account of the absence of foreign light, even more distinctly than when they are in the same right line with that object. And if we remove the plates to a considerable distance out of this line, the rings are still visible, and become larger than before; for here the actual route of the light passing through the air, is longer than that of the light passing more obliquely through the water, and the difference in the times of passage is lessened. It is however impossible to be quite confident with respect to the causes of these minute variations, without some means of ascertaining accurately the forms of the dissipating surfaces.

In applying the general law of interference to these colours, as well as to those of thin plates already known, I must confess that it is impossible to avoid another supposition, which is a part of the undulatory theory, that is, that the velocity of light is the greater, the rarer the medium; and that there is also a condition annexed to the explanation of the colours of thin plates, which involves another part of the same theory, that is,

that where one of the portions of light has been reflected at the surface of a rarer medium, it must be supposed to be retarded one half of the appropriate interval, for instance in the central black spot of a soap-bubble, where the actual lengths of the paths very nearly coincide, but the effect is the same as if one of the portions had been so retarded as to destroy the other. From considering the nature of this circumstance, I ventured to predict, that if the two reflections were of the same kind, made at the surfaces of a thin plate, of a density intermediate between the densities of the mediums containing it, the effect would be reversed, and the central spot, instead of black, would become white; and I have now the pleasure of stating, that I have fully verified this prediction, by interposing a drop of oil of sassafras between a prism of flint-glass and a lens of crown-glass: the central spot seen by reflected light was white, and surrounded by a dark ring. It was however necessary to use some force, in order to produce a contact sufficiently intimate; and the white spot differed, even at last, in the same degree from perfect whiteness, as the black spot usually does from perfect blackness.

The colours of mixed plates suggested to me an idea which appears to lead to an explanation of the dispersion of colours by refraction, more simple and satisfactory than that which I advanced in the last Bakerian Lecture. We may suppose that every refractive medium transmits the undulations constituting light in two separate portions, one passing through its ultimate particles, and the other through its pores; and that these portions re-unite continually, after each successive separation, the one having preceded the other by a very minute but constant interval, depending on the regular arrangement of the particles of a homogeneous medium. Now, if these two portions were always equal, each point of the undulations resulting from their re-union would always be found half-way between the places of the corresponding point in the separate portions; but supposing the preceding portion to be the smaller, the newly combined undulation will be less advanced than if both had been equal, and the difference of its place will depend, not only on the difference of the length of the two routes, which will be

constant for all the undulations; but also on the law and magnitude of those undulations; so that the larger undulations will be somewhat further advanced after each re-union than the smaller ones, and, the same operation recurring at every particle of the medium, the whole progress of the larger undulations will be more rapid than that of the smaller; hence the deviation, in consequence of the retardation of the motion of light in a denser medium, will of course be greater for the smaller than for the larger undulations. Assuming the law of the harmonic curve for the motions of the particles, we might without much difficulty reduce this conjecture to a comparison with experiment; but it would be necessary, in order to warrant our conclusions, to be provided with very accurate measures of the refractive and dispersive powers of various substances, for rays of all descriptions.

Dr. Wollaston's very interesting observations would furnish great assistance in this inquiry, when compared with the separation of colours by thin plates. I have repeated his experiments on the spectrum with perfect success, and have made some attempts to procure comparative measures from thin plates; and I have found that, as Sir Isaac Newton has already observed, the blue and violet light is more dispersed by refraction, than in proportion to the difference of the appropriate dimensions deduced from the phenomena of thin plates. Hence it happens, that when a line of the light proceeding to form an image of the rings of colours of thin plates, is intercepted by a prism, and an actual picture is formed, resembling the scale delineated by Newton from theory, for estimating the colours of particles of given dimensions, the oblique spectrums, formed by the different colours of each series, are not straight, but curved, the lateral refraction of the prism separating the violet end more widely than the red. The thickness corresponding to the extreme red, the line of yellow, bright green, bright blue, and extreme violet, I found to be inversely as the numbers 27, 30, 35, 40, and 45, respectively. In consequence of Dr. Wollaston's correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last Bakerian Lec-

ture, respecting the proportions of the sympathetic fibres of the retina; substituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6.

The same prismatic analysis of the colours of thin plates, appears to furnish a satisfactory explanation of the subdivision of the light of the lower part of a candle; for, in fact, the light transmitted through every part of a thin plate, is divided in a similar manner into distinct portions, increasing in number with the thickness of the plate, until they become too minute to be visible. At the thickness corresponding to the ninth or tenth portion of red light, the number of portions of different colours is five; and their proportions, as exhibited by refraction, are nearly the same as in the light of a candle, the violet being the broadest. We have only to suppose each particle of tallow to be, at its first evaporation, of such dimensions as to produce the same effect as the thin plate of air at this point, where it is about $\frac{1}{10000}$ of an inch in thickness, and to reflect, or perhaps rather to transmit, the mixed light produced by the incipient combustion around it, and we shall have a light completely resembling that which Dr. Wollaston has observed. There appears to be also a fine line of strong yellow light, separate from the general spectrum, principally derived from the most superficial combustion at the margin of the flame, and increasing in quantity as the flame ascends. Similar circumstances might undoubtedly be found in other cases of the production or modification of light; and experiments upon this subject might tend greatly to establish the Newtonian opinion, that the colours of all natural bodies are similar in their origin to those of thin plates; an opinion which appears to do the highest honour to the sagacity of its author, and indeed to form a very considerable step in our advances towards an acquaintance with the intimate constitution and arrangement of material substances.

I have lately had an opportunity of confirming my former observations on the dispersive powers of the eye. I find that, at the respective distances of 10 and 15 inches, the extreme red and extreme violet rays are similarly refracted, the difference being expressed by a focal length of 30 inches. Now the interval between red and yellow is about one-fourth of the whole

spectrum; consequently, a focal length of 120 inches expresses a power equivalent to the dispersion of the red and yellow, and this differs but little from 132, which was the result of the observation already described. I do not know that these experiments are more accurate than the former one; but I have repeated them several times under different circumstances, and I have no doubt that the dispersion of coloured light in the human eye is nearly such as I have stated it. How it happens to be no greater, I cannot at present undertake to explain.

No. IX.

EXPERIMENTS AND CALCULATIONS RELATIVE TO

PHYSICAL OPTICS.

From the Philosophical Transactions for 1804.

A BAKERIAN LECTURE.

READ NOV. 24, 1803.

I.—*Experimental Demonstration of the general Law of the Interference of Light.*

IN making some experiments on the fringes of colours accompanying shadows, I have found so simple and so demonstrative a proof of the general law of the interference of two portions of light, which I have already endeavoured to establish, that I think it right to lay before the Royal Society a short statement of the facts which appear to me so decisive. The proposition on which I mean to insist, at present, is simply this—that fringes of colours are produced by the interference of two portions of light; and I think it will not be denied by the most prejudiced, that the assertion is proved by the experiments I am about to relate, which may be repeated with great ease whenever the sun shines, and without any other apparatus than is at hand to every one.

Exper. 1. I made a small hole in a window-shutter, and covered it with a piece of thick paper, which I perforated with a fine needle. For greater convenience of observation I placed a small looking-glass without the window-shutter, in such a position as to reflect the sun's light, in a direction nearly horizontal, upon the opposite wall, and to cause the cone of diverging light to pass over a table on which were several little screens of

card-paper. I brought into the sunbeam a slip of card, about one-thirtieth of an inch in breadth, and observed its shadow, either on the wall or on other cards held at different distances. Besides the fringes of colour on each side of the shadow, the shadow itself was divided by similar parallel fringes, of smaller dimensions, differing in number, according to the distance at which the shadow was observed, but leaving the middle of the shadow always white. Now these fringes were the joint effects of the portions of light passing on each side of the slip of card, and inflected, or rather diffracted, into the shadow. For, a little screen being placed a few inches from the card, so as to receive either edge of the shadow on its margin, all the fringes which had before been observed in the shadow on the wall, immediately disappeared, although the light inflected on the other side was allowed to retain its course, and although this light must have undergone any modification that the proximity of the other edge of the slip of card might have been capable of occasioning. When the interposed screen was more remote from the narrow card, it was necessary to plunge it more deeply into the shadow, in order to extinguish the parallel lines; for here the light, diffracted from the edge of the object, had entered further into the shadow in its way towards the fringes. Nor was it for want of a sufficient intensity of light that one of the two portions was incapable of producing the fringes alone; for, when they were both uninterrupted, the lines appeared, even if the intensity was reduced to one-tenth or one-twentieth.

Exper. 2. The crested fringes described by the ingenious and accurate Grimaldi, afford an elegant variation of the preceding experiment, and an interesting example of a calculation grounded on it. When a shadow is formed by an object which has a rectangular termination, besides the usual external fringes, there are two or three alternations of colours, beginning from the line which bisects the angle, disposed on each side of it in curves, which are convex towards the bisecting line, and which converge in some degree towards it, as they become more remote from the angular point. These fringes are also the joint effect of the light which is inflected directly towards the shadow from each of the two outlines of the object; for if a screen be placed

within a few inches of the object, so as to receive only one of the edges of the shadow, the whole of the fringes disappear: if, on the contrary, the rectangular point of the screen be opposed to the point of the shadow, so as barely to receive the angle of the shadow on its extremity, the fringes will remain undisturbed.

II.—*Comparison of Measures deduced from various Experiments.*

If we now proceed to examine the dimensions of the fringes, under different circumstances, we may calculate the differences of the lengths of the paths described by the portions of light which have thus been proved to be concerned in producing those fringes; and we shall find that, where the lengths are equal, the light always remains white; but that, where either the brightest light, or the light of any given colour, disappears and reappears, a first, a second, or a third time, the differences of the lengths of the paths of the two portions are in arithmetical progression, as nearly as we can expect experiments of this kind to agree with each other. I shall compare, in this point of view, the measures deduced from several experiments of Newton, and from some of my own.

In the eighth and ninth observations of the third book of Newton's Optics, some experiments are related, which, together with the third observation, will furnish us with the data necessary for the calculation. Two knives were placed, with their edges meeting at a very acute angle, in a beam of the sun's light, admitted through a small aperture, and the point of concourse of the two first dark lines bordering the shadows of the respective knives was observed at various distances. The results of six observations are expressed in the first three lines of the first Table. On the supposition that the dark line is produced by the first interference of the light reflected from the edges of the knives, with the light passing in a straight line between them, we may assign, by calculating the difference of the two paths, the interval for the first disappearance of the brightest light, as it is expressed in the fourth line. The second Table contains the results of a similar calculation, from Newton's observations on the shadow of a hair; and the third, from some experiments of

my own, of the same nature; the second bright line being supposed to correspond to a double interval, the second dark line to a triple interval, and the succeeding lines to depend on a continuation of the progression. The unit of all the tables is an inch.

TABLE I. *Obs.* 9. N.

Distance of the knives from the aperture						101
Distance of the paper from the knives	$1\frac{1}{2}$,	$3\frac{1}{3}$,	$8\frac{3}{8}$,	32,	96,	131
Distances between the edges of the knives, opposite to the point of concourse	.012,	.020,	.034,	.057,	.081,	.087
Interval of disappearance	.0000122,	.0000155,	.0000182,	.0000167,	.0000166,	.0000166

TABLE II. *Obs.* 3. N.

Breadth of the hair		$\frac{1}{280}$
Distance of the hair from the aperture		144
Distances of the scale from the aperture	150,	252
(Breadths of the shadow	$\frac{1}{54}$,	$\frac{1}{5}$)
Breadth between the second pair of bright lines	$\frac{2}{77}$,	$\frac{4}{17}$
Interval of disappearance, or half the difference of the paths	.0000151,	.0000173
Breadth between the third pair of bright lines	$\frac{4}{73}$,	$\frac{3}{10}$
Interval of disappearance, one-fourth of the difference	.0000130,	.0000143

TABLE III. *Exper.* 3.

Breadth of the object	.434
Distance of the object from the aperture	125
Distance of the wall from the aperture	250
Distance of the second pair of dark lines from each other	1.167
Interval of disappearance, one-third of the difference	.0000149

Exper. 4.

Breadth of the wire	.083
Distance of the wire from the aperture	32
Distance of the wall from the aperture	250
(Breadth of the shadow, by three measurements	.815, .826, or .827; mean, .823)
Distance of the first pair of dark lines	1.165, 1.170, or 1.160; mean, 1.165
Interval of disappearance	.0000194
Distance of the second pair of dark lines	1.402, 1.395, or 1.400; mean, 1.399
Interval of disappearance	.0000137
Distance of the third pair of dark lines	1.594, 1.580, or 1.585; mean, 1.586
Interval of disappearance	.0000128

It appears, from five of the six observations of the first Table, in which the distance of the shadow was varied from about 3 inches to 11 feet, and the breadth of the fringes was increased in the ratio of 7 to 1, that the difference of the routes constituting the interval of disappearance, varied but one-eleventh at most; and that, in three out of the five, it agreed with the mean, either exactly, or within $\frac{1}{100}$ part. Hence we are warranted in inferring that the interval appropriate to the extinction of the brightest light, is either accurately or very nearly constant.

But it may be inferred, from a comparison of all the other observations, that when the obliquity of the reflection is very great, some circumstance takes place, which causes the interval thus calculated to be somewhat greater: thus, in the eleventh line of the third Table, it comes out one-sixth greater than the mean of the five already mentioned. On the other hand, the mean of two of Newton's experiments and one of mine, is a result about one-fourth less than the former. With respect to the nature of this circumstance, I cannot at present form a decided opinion; but I conjecture that it is a deviation of some of the light concerned, from the rectilinear direction assigned to it, arising either from its natural diffraction, by which the magnitude of the shadow is also enlarged, or from some other unknown cause. If we imagined the shadow of the wire, and the fringes nearest it, to be so contracted that the motion of the light bounding the shadow might be rectilinear, we should thus make a sufficient compensation for this deviation; but it is difficult to point out what precise track of the light would cause it to require this correction.

The mean of the three experiments which appear to have been least affected by this unknown deviation, gives .0000127 for the interval appropriate to the disappearance of the brightest light; and it may be inferred that if they had been wholly exempted from its effects, the measure would have been somewhat smaller. Now the analogous interval, deduced from the experiments of Newton on thin plates, is .0000112, which is about one-eighth less than the former result; and this appears to be a coincidence fully sufficient to authorise us to attribute these two classes of phenomena to the same cause. It is very

easily shown, with respect to the colours of thin plates, that each kind of light disappears and reappears where the differences of the routes of two of its portions are in arithmetical progression; and we have seen that the same law may be in general inferred from the phenomena of diffracted light, even independently of the analogy.

The distribution of the colours is also so similar in both cases, as to point immediately to a similarity in the causes. In the thirteenth observation of the second part of the first book, Newton relates, that the interval of the glasses where the rings appeared in red light, was to the interval where they appeared in violet light, as 14 to 9; and, in the eleventh observation of the third book, that the distances between the fringes, under the same circumstances, were the 22d and 27th of an inch. Hence, deducting the breadth of the hair and taking the squares, in order to find the relation of the difference of the routes, we have the proportion of 14 to $9\frac{1}{4}$, which scarcely differs from the proportion observed in the colours of the thin plate.

We may readily determine, from this general principle, the form of the crested fringes of Grimaldi, already described; for it will appear that, under the circumstances of the experiment related, the points in which the differences of the lengths of the paths described by the two portions of light are equal to a constant quantity, and in which, therefore, the same kinds of light ought to appear or disappear, are always found in equilateral hyperbolas, of which the axes coincide with the outlines of the shadow, and the asymptotes nearly with the diagonal line. Such, therefore, must be the direction of the fringes; and this conclusion agrees perfectly with the observation. But it must be remarked, that the parts near the outlines of the shadow are so much shaded off, as to render the character of the curve somewhat less decidedly marked where it approaches to its axis. These fringes have a slight resemblance to the hyperbolic fringes observed by Newton; but the analogy is only distant.

III.—*Application to the Supernumerary Rainbows.*

The repetitions of colours sometimes observed within the common rainbow, and described in the Philosophical Transactions, by Dr. Langwith and Mr. Daval, admit also a very easy and complete explanation from the same principles, Dr. Pemberton has attempted to point out an analogy between these colours and those of thin plates; but the irregular reflection from the posterior surface of the drop, to which alone he attributes the appearance, must be far too weak to produce any visible effects. In order to understand the phenomenon, we have only to attend to the two portions of light which are exhibited in the common diagrams explanatory of the rainbow, regularly reflected from the posterior surface of the drop, and crossing each other in various directions, till, at the angle of the greatest deviation, they coincide with each other, so as to produce, by the greater intensity of this redoubled light, the common rainbow of 41 degrees. Other parts of these two portions will quit the drop in directions parallel to each other; and these would exhibit a continued diffusion of fainter light, for 25° within the bright termination which forms the rainbow, but for the general law of interference, which, as in other similar cases, divides the light into concentric rings; the magnitude of these rings depending on that of the drop, according to the difference of time occupied in the passage of the two portions, which thus proceed in parallel directions to the spectator's eye, after having been differently refracted and reflected within the drop. This difference varies, at first, nearly as the square of the angular distance from the primitive rainbow; and, if the first additional red be at the distance of 2° from the red of the rainbow, so as to interfere a little with the primitive violet, the fourth additional red will be at a distance of nearly 2° more; and the intermediate colours will occupy a space nearly equal to the original rainbow. In order to produce this effect, the drops must be about $\frac{1}{77}$ of an inch, or .013, in diameter: it would be sufficient if they were between $\frac{1}{77}$ and $\frac{1}{88}$. The reason that such supernumerary colours are not often seen, must be, that it does not often happen that drops so nearly equal are found together; but, that this may some-

times happen, is not in itself at all improbable: we measure even medicines by dropping them from a phial, and it may easily be conceived that the drops formed by natural operations may sometimes be as uniform as any that can be produced by art. How accurately this theory coincides with the observation, may best be determined from Dr. Langwith's own words.

"August the 21st, 1722, about half an hour past five in the evening, weather temperate, wind at north-east, the appearance was as follows:—The colours of the primary rainbow were as usual, only the purple very much inclining to red, and well defined: under this was an arch of green, the upper part of which inclined to a bright yellow, the lower to a more dusky green: under this were alternately two arches of reddish purple, and two of green: under all, a faint appearance of another arch of purple, which vanished and returned several times so quick, that we could not readily fix our eyes upon it. Thus the order of the colours was, I. Red, orange-colour, yellow, green, light-blue, deep blue, purple. II. Light green, dark green, purple. III. Green, purple. IV. Green, faint vanishing purple. You see we had here four orders of colours, and perhaps the beginning of a fifth: for I make no question but that what I call the purple, is a mixture of the purple of each of the upper series with the red of the next below it, and the green a mixture of the intermediate colours. I send you not this account barely upon the credit of my own eyes; for there was a clergyman and four other gentlemen in company, whom I desired to view the colours attentively, who all agreed that they appear in the manner that I have now described. There are two things which well deserve to be taken notice of, as they may perhaps direct us, in some measure, to the solution of this curious phenomenon. The first is, that the breadth of the first series so far exceeded that of any of the rest, that, as near as I could judge, it was equal to them all taken together. The second is, that I have never observed these inner orders of colours in the lower parts of the rainbow, though they have often been incomparably more vivid than the upper parts, under which the colours have appeared. I have taken notice of this so very often, that I can hardly look upon it to be accidental;

and, if it should prove true in general, it will bring the disquisition into a narrow compass; for it will show that this effect depends upon some property which the drops retain, whilst they are in the upper part of the air, but lose as they come lower, and are more mixed with one another." Phil. Trans., Vol. XXXII. p. 243.

From a consideration of the nature of the secondary rainbow, of 54°, it may be inferred, that if any such supernumerary colours were seen attending this rainbow, they would necessarily be external to it, instead of internal. The circles sometimes seen encompassing the observer's shadow in a mist, are perhaps more nearly related to the common colours of thin plates as seen by reflection.

IV.—*Argumentative Inference respecting the Nature of Light.*

The experiment of Grimaldi, on the crested fringes within the shadow, together with several others of his observations, equally important, has been left unnoticed by Newton. Those who are attached to the Newtonian theory of light, or to the hypothesis of modern opticians, founded on views still less enlarged, would do well to endeavour to imagine any thing like an explanation of these experiments, derived from their own doctrines; and, if they fail in the attempt, to refrain at least from idle declamation against a system which is founded on the accuracy of its application to all these facts, and to a thousand others of a similar nature.

From the experiments and calculations which have been premised, we may be allowed to infer, that homogeneous light, at certain equal distances in the direction of its motion, is possessed of opposite qualities, capable of neutralising or destroying each other, and of extinguishing the light, where they happen to be united; that these qualities succeed each other alternately in successive concentric superficies, at distances which are constant for the same light, passing through the same medium. From the agreement of the measures, and from the similarity of the phenomena, we may conclude, that these intervals are the same as are concerned in the production of the colours of thin plates;

but these are shown, by the experiments of Newton, to be the smaller, the denser the medium; and, since it may be presumed that their number must necessarily remain unaltered in a given quantity of light, it follows of course, that light moves more slowly in a denser, than in a rarer medium; and this being granted, it must be allowed, that refraction is not the effect of an attractive force directed to a denser medium. The advocates for the projectile hypothesis of light, must consider which link in this chain of reasoning they may judge to be the most feeble; for, hitherto, I have advanced in this Paper no general hypothesis whatever. But, since we know that sound diverges in concentric superficies, and that musical sounds consist of opposite qualities, capable of neutralising each other, and succeeding at certain equal intervals, which are different according to the difference of the note, we are fully authorized to conclude, that there must be some strong resemblance between the nature of sound and that of light.

I have not, in the course of these investigations, found any reason to suppose the presence of such an inflecting medium in the neighbourhood of dense substances as I was formerly inclined to attribute to them; and, upon considering the phenomena of the aberration of the stars, I am disposed to believe that the luminiferous ether pervades the substance of all material bodies with little or no resistance, as freely perhaps as the wind passes through a grove of trees.

The observations on the effects of diffraction and interference may perhaps sometimes be applied to a practical purpose, in making us cautious in our conclusions respecting the appearances of minute bodies viewed in a microscope. The shadow of a fibre, however opaque, placed in a pencil of light admitted through a small aperture, is always somewhat less dark in the middle of its breadth than in the parts on each side. A similar effect may also take place, in some degree, with respect to the image on the retina, and impress the sense with an idea of a transparency which has no real existence: and if a small portion of light be really transmitted through the substance, this may again be destroyed by its interference with the diffracted light, and produce an appearance of partial opacity, instead of

uniform semi-transparency. Thus a central dark spot and a light spot, surrounded by a darker circle, may respectively be produced in the images of a semi-transparent and an opaque corpuscle; and impress us with an idea of a complication of structure which does not exist. In order to detect the fallacy, we may make two or three fibres cross each other, and view a number of globules contiguous to each other; or we may obtain a still more effectual remedy by changing the magnifying power; and then, if the appearance remain constant in kind and in degree, we may be assured that it truly represents the nature of the substance to be examined. It is natural to inquire whether or no the figures of the globules of blood, delineated by Mr. Hewson in the Phil. Trans., Vol. LXIII. for 1773, might not in some measure have been influenced by a deception of this kind: but, as far as I have hitherto been able to examine the globules, with a lens of one-fiftieth of an inch focus, I have found them nearly such as Mr. Hewson has described them.

V.—*Remarks on the Colours of Natural Bodies.*

Exper. 5. I have already adduced, in illustration of Newton's comparison of the colours of natural bodies with those of thin plates, Dr. Wollaston's observations on the blue light of the lower part of a candle, which appears, when viewed through a prism, to be divided into five portions. I have lately observed a similar instance, still more strongly marked, in the light transmitted by the blue glass sold by the opticians. This light is separated by the prism into seven distinct portions, nearly equal in magnitude, but somewhat broader, and less accurately defined, towards the violet end of the spectrum. The first two are red, the third is yellowish green, the fourth green, the fifth blue, the sixth bluish violet, and the seventh violet. This division agrees very nearly with that of the light reflected by a plate of air $\frac{1}{8840}$ of an inch in thickness, corresponding to the 11th series of red, and the 18th of violet. A similar plate of a metallic oxide would perhaps be about $\frac{1}{13000}$ of an inch in thickness. But it must be confessed that there are strong reasons for believing the colouring particles of

natural bodies in general to be incomparably smaller than this; and it is probable that the analogy suggested by Newton is somewhat less close than he imagined. The light reflected by a plate of air, at any thickness nearly corresponding to the 11th red, appears to the eye to be very nearly white; but, under favourable circumstances, the 11th red and the neighbouring colours may still be distinguished. The light of some kinds of coloured glass is pure red; that of others red with a little green: some intercept all the light, except the extreme red and the blue. In the blue light of a candle, expanded by the prism, the portions of each colour appear to be narrower, and the intervening dark spaces wider than in the analogous spectrum derived from the light reflected from a thin plate. The light of burning alcohol appears to be green and violet only. The pink dye sold in the shops, which is a preparation of the carthamus, affords a good specimen of a yellow green light regularly reflected, and a crimson probably produced by transmission.

VI.—*Experiment on the Dark Rays of Ritter.*

Exper. 6. The existence of solar rays accompanying light, more refrangible than the violet rays, and cognisable by their chemical effects, was first ascertained by Mr. Ritter; but Dr. Wollaston made the same experiments a very short time afterwards, without having been informed of what had been done on the Continent. These rays appear to extend beyond the violet rays of the prismatic spectrum, through a space nearly equal to that which is occupied by the violet. In order to complete the comparison of their properties with those of visible light, I was desirous of examining the effect of their reflection from a thin plate of air, capable of producing the well-known rings of colours. For this purpose I formed an image of the rings, by means of the solar microscope, with the apparatus which I have described in the Journals of the Royal Institution, and I threw this image on paper dipped in a solution of nitrate of silver, placed at the distance of about nine inches from the microscope. In the course of an hour portions of three dark rings were very distinctly visible, much smaller than the

brightest rings of the coloured image, and coinciding very nearly, in their dimensions, with the rings of violet light that appeared upon the interposition of violet glass. I thought the dark rings were a little smaller than the violet rings, but the difference was not sufficiently great to be accurately ascertained; it might be as much as $\frac{1}{30}$ or $\frac{1}{40}$ of the diameters, but not greater. It is the less surprising that the difference should be so small, as the dimensions of the coloured rings do not by any means vary at the violet end of the spectrum so rapidly as at the red end. For performing this experiment with very great accuracy a heliostate would be necessary, since the motion of the sun causes a slight change in the place of the image; and leather, impregnated with the muriate of silver, would indicate the effect with greater delicacy. The experiment, however, in its present state, is sufficient to complete the analogy of the invisible with the visible rays, and to show that they are equally liable to the general law which is the principal subject of this Paper. If we had thermometers sufficiently delicate, it is probable that we might acquire, by similar means, information still more interesting, with respect to the rays of invisible heat discovered by Dr. Herschel; but at present there is great reason to doubt of the practicability of such an experiment.

No. X.

DR. YOUNG'S REPLY TO THE ANIMADVERSIONS OF THE

EDINBURGH REVIEWERS,

ON SOME PAPERS PUBLISHED IN THE PHILOSOPHICAL TRANSACTIONS.*

Illud oro: si meam in omni vita, tum in dicendo, moderationem modestiamque cognostis, ne me hodie, cum isti, ut provocavit, respondero, oblitum esse putetis mei.—CIC.

A MAN who has a proper regard for the dignity of his own character, although his sensibility may sometimes be awakened by the unjust attacks of interested malevolence, will esteem it in general more advisable to bear, in silence, the temporary effects of a short-lived injury, than to suffer his own pursuits to

* The three preceding Memoirs, Nos. VII., VIII., and IX. which established the bases of the most important advancement which the science of Physical Optics had made since the time of Newton, were attacked with great violence, soon after their appearance, in Nos. II. and IX. of the Edinburgh Review. These criticisms have been commonly attributed to Lord Brougham, and are probably not surpassed in wit, sarcasm, and power by any other productions of that distinguished writer. It was unfortunate, however, that they should have been devoted to the support of views which have been proved to be, nearly in every instance, erroneous. Their influence, however, upon public opinion was more remarkable than could reasonably have been expected, even from the great authority of the publication in which they appeared, and the unquestionable ability with which they were written. They not only seriously damaged, for the time, the estimation of the scientific character of Dr. Young, but diverted public attention from the examination of the truth of his theories, at least amongst his own countrymen, for nearly twenty years. It was to M. Arago that the credit is due of having first fully recognised and proclaimed their value, in connection with his own researches and those of his illustrious friend, M. Fresnel.

The following passages of these Reviews will sufficiently show the spirit in which they were written:—

"As this paper (the Bakerian Lecture on the Theory of Light and Colours, No. VII.) contains nothing which deserves the name, either of experiment or discovery, and as it is in fact destitute of every species of merit, we should have allowed it to pass among the multitude of those articles which must always find admittance into the collections of a Society which is pledged to publish two or three volumes every year. The dignities of the author, and the title of Bakerian Lecture, which is prefixed to these lucubrations, should not have saved them from a place in the ignoble crowd. But we have of late observed in the physical world a most unaccountable predilection for vague hypothesis daily gaining ground; and we are mortified to see that the Royal Society, forgetful of those improvements in science to which it owes

be interrupted, in making an effort to repel the invective, and to punish the aggressor. But it is possible that art and malice may be so insidiously combined, as to give to the grossest misrepresentations the semblance of justice and candour; and,

its origin, and neglecting the precepts of its most illustrious members, is now, by the publication of such papers, giving the countenance of its highest authority to dangerous relaxations in the principles of physical logic. We wish to raise our feeble voice against innovations that can have no other effect than to check the progress of Science, and renew all those wild phantoms of the imagination which Bacon and Newton put to flight from her temple. We wish to recall philosophers to the strict and severe methods of investigation pointed out by the transcendent talents of those illustrious men, and consecrated by their astonishing success; and, for this purpose, we take the first opportunity that has been presented to us, of calling our readers' attention to this mode of philosophising, which seems, by the title of the paper now before us, to have been honoured with more than the ordinary approbation of the Council."

The reviewer, in noticing the second paper (An Account of some Cases of the Production of Colours, No. VIII.), after giving his own explanation of a special case of the law of interference, in opposition to that of Dr. Young, concludes with the following admonition to the Royal Society:—

"We cannot conclude our review of these articles without entreating, for a moment, the attention of that illustrious body, which has admitted of late years so many paltry and unsubstantial papers into its Transactions. Great as the services are which the Royal Society has rendered to the world, and valuable as the papers have been in every volume (not less valuable, surely, since the accession of the present excellent President), we think on the benefits which it has conferred with feelings of the warmest gratitude. We only wish that those feelings should be unmingled by any ideas of regret, from the want of selection to which we are adverting; and that it should cease to give its countenance to such vain theories as those which we find mingled, in this volume, with a vast body of important information. The Society has, indeed, been in the habit of stating, that the truth and other merits of the speculations which it publishes must rest with their respective authors; but we are afraid this is not sufficient. The Society publishes these papers—meets for the purpose of reading them—calls them its Transactions; and, in fact, exercises, in many cases, the power of rejecting the papers which are offered. It is in vain, therefore, to disavow a responsibility which so many circumstances concur in fixing. The public will always look upon the Society as immediately responsible for the papers which compose its Transactions, unless, indeed, it shall wish to be degraded into the rank of a mere mechanical contrivance for the printing of miscellanies. We implore the Council, therefore, if they will deign to cast their eyes upon our humble page, to prevent a degradation of the Institution which has so long held the first rank among scientific bodies. Let them reflect on the mighty name which has been transmitted to them—

——— 'Clarum et venerabile nomen
Gentibus, et multum nostræ quod proderat urbi.

Such a name may indeed shelter them in their weakness, and make us venerate, even in the frailty of old age, an institution illustrious for its ancient virtue. But is it impossible to ward off the encroachments of time, and to renovate, in new achievements, the vigour of former years? It is more honourable to support an illustrious character, than to appeal to it for shelter and protection."

In the third and last of these Reviews, after further exposing "the law of interference," as absurd and illogical, and endeavouring to explain away the more important and decisive experiments upon which it was founded, he thus closes the whole controversy: "We now dismiss, for the present, the feeble lucubrations of this author, in which we have searched without success for some traces of learning, acuteness, and ingenuity, that might compensate his evident deficiency in the powers of solid thinking, calm and patient investigation, and successful development of the laws of Nature, by

especially where the subject of the discussion is of a nature little adapted to the comprehension of the generality of readers, even a man's friends may be so far misled by a garbled extract from his own works, and by the specious mixture of partial truth with essential falsehood, that they may not only be unable to defend him from the unfavourable opinion of others, but may themselves be disposed to suspect, in spite of their partiality, that he has been hasty and inconsiderate at least, if not radically weak and mistaken. In such a case, he owes to his friends such explanations as will enable them to see clearly the injustice of the accusation, and the iniquity of its author: and, if he is in a situation which requires that he should in a certain degree possess the public confidence, he owes to himself and to the public to prove, that the charges of imbecility of mind and perversity of disposition are not more founded with regard to him, than with regard to all who are partakers with him in the unavoidable imperfections of human nature.

Precisely such is my situation. I have at various times communicated to the Royal Society, in a very abridged form, the results of my experiments and investigations, relating to different branches of natural philosophy: and the Council of the Society, with a view perhaps of encouraging patient diligence, has honoured my essays with a place in their Transactions. Several of these essays have been singled out, in an unprecedented manner, from the volumes in which they were printed, and have been made the subjects, in the second and ninth numbers of the Edinburgh Review, not of criticism, but of ridicule and invective; of an attack, not only upon my writings and my literary pursuits, but almost on my moral character. The peculiarity of the style and tendency of this attack led me

steady and modest observation of her operations. We came to the examination with no other prejudice than the very allowable prepossession against vague hypothesis, by which all true lovers of science have for above a century and a half been swayed. We pursued it, both on the present and on a former occasion, without any feelings except those of regret at the abuse of that time and opportunity which no greater share of talents than Dr. Young's are sufficient to render fruitful by mere diligence and moderation. From us, however, he cannot claim any portion of respect until he shall alter his mode of proceeding, or change the subject of his lucubrations; and we feel ourselves more particularly called upon to express our disapprobation, because, as distinction has been unwarily bestowed on his labours by the most illustrious of scientific bodies, it is the more necessary that a free protest should be recorded before the more humble tribunals of literature."—*Note by the Editor.*

at once to suspect, that it must have been suggested by some other motive than the love of truth; and I have both internal and external evidence for believing, that the articles in question are, either wholly, or in great measure, the productions of an individual, upon whose mathematical works I had formerly thought it necessary to make some remarks, which, though not favourable, were far from being severe;* and whose optical speculations, partly confuted before, and already forgotten, appeared, to their fond parent, to be in danger of a still more complete rejection from the establishment of my opinions. As far as my reputation in natural philosophy is concerned, I should consider a libel of this kind as neither requiring nor deserving an answer; but I cannot help feeling the propriety of endeavouring to defend myself from the more pernicious influence of those imputations, which might tend to lessen the confidence of the public in the professional qualifications of a man, whose abilities have been thus contemptuously and repeatedly depreciated. The practice of physic has always been, either immediately or remotely, the object of my pursuits, and I can affirm, without fear of contradiction, that I have never neglected any opportunity either of improving myself in its study, or of being useful to the humblest of those who have committed themselves to my care, in its application. But I have no right to expect that any degree of industry that I may have employed, should encourage a man to intrust me with the management of that which so nearly concerns his happiness and prosperity, if he has reason to think me rash, and vain, and wavering in my opinions, and that even upon subjects which are generally supposed to admit of proofs perfectly decisive and satisfactory.

My Bakerian lecture on the theory of light and colours, and another paper published in the same volume of the Philosophical Transactions, are the subjects of two of the most scurrilous articles in the second number of the Edinburgh Review. The writer of these articles has, as a prelude to his imputation of a "vibratory and undulatory mode of reasoning," very unnecessarily recurred to the first essay that I presented to the

* Supra, p. 101; see also note at the foot of p. 99.—*Note by the Editor.*

Royal Society, as long ago as the year 1793; I am therefore obliged to explain the circumstances which led me to the subject of that essay, and to relate the history of my opinions concerning it: and as he has thought proper to insinuate, in the form of insolent admonition, that I have never studied even "the plainer parts" of the works of Newton, I must state when and why I actually read those admirable productions; and I shall think it right to account, at the same time, for the manner in which, as a medical man, I have been led, for a time, into the extensive regions of natural philosophy.

It is now more than fourteen years since I first resolved to devote my life to the profession of physic. I continued for two years the pursuit of those attainments, in mathematics and in general literature, which had before constituted my sole occupation, and which, by the express sentiment of the father of the medical sciences, and by the universal suffrages of the more liberal part of mankind, have been allowed to be the surest and best foundations for the superstructure of the requisite qualifications of a physician. The causes of disease, obscure in their nature, and hidden in their operation, elude but too frequently the most diligent researches of the strongest and most experienced minds: they afford ample scope to the most minute investigation, and the most sagacious discernment; but they require that the faculties of the observer should have been sufficiently prepared, by being employed on subjects of a nature more certainly definable, and more perfectly intelligible. Classical literature, mathematical philosophy, chemistry and natural history, a knowledge of different countries, and an acquaintance with different languages, are as necessary to the melioration of those powers of reasoning which are to be called into activity in the pursuit of a profession, as they are essential to the perfection of the character of a general scholar, and an accomplished man. This must be my excuse for having devoted a considerable portion of my attention to the study of the classics, on my success in which the Edinburgh Reviewers have, with an insulting affectation of candour, thought fit, on another occasion, to compliment me. I pursued the study of mathematics and natural philosophy as far only as I esteemed them

subservient to other objects: not that I preferred philology to science, but because I thought myself obliged to sacrifice both to physic. After having rendered myself familiar with many other mathematical works, I read, in the autumn of 1790, both the Principia of Newton and his Optics. I read not the "plainest parts of the Principia" only, but the whole; and all that the illustrious author meant to be understood by a reader, I understood and admired: where he purposely omitted a demonstration, I did not at that time attempt to investigate it. That I was then satisfied with some few parts which I do not now think unexceptionable, might easily have happened, even if I had felt less reverence than I have uniformly done for the character of the unrivalled author. The Optics too I read with attention and delight, yet by no means with the same satisfaction that I had derived from the perusal of the Principia.

My attention to optical subjects was not revived till the year 1793, when, in the course of my anatomical studies, the theory of vision was necessarily to be reconsidered. I saw, what I then thought none had seen before, that the crystalline lens was of a fibrous structure; and I could find no other satisfactory mode of explaining the phenomena of vision than by attributing to it muscular powers. On this subject I presented a short paper to the Royal Society,* to which, from the circumstance of the late Mr. Hunter's reclamation of the discovery as his own, a greater degree of novelty was imputed than it perhaps deserved. Mr. Home too attributed to Mr. Hunter the merit of a discovery "not small nor unimportant," that of an animal in which the fibrous structure of the lens was easily traced. I had however found no difficulty in observing, in the eye of a quadruped, the arrangement which had been the basis of my speculations.

It was in the course of the winter which I spent in pursuing my medical studies at Edinburgh, that I first read Mr. Home's account of his experiments on vision.† This investigation convinced Mr. Home that Mr. Hunter, whose sentiments he had

* No. I., p. 1.

† The Croonian lecture on Muscular Motion. Phil. Trans. for 1794, vol. lxxxiv., p. 1.

before adopted, was mistaken in his opinion; and when I had afterwards seen at Göttingen Dr. Olbers' elegant dissertation on the same subject, I found it impossible to resist, without making further experiments of my own, the appearance of evidence which was brought against my favourite opinion. I had not then learned of the Edinburgh Reviewers how much easier it was to deny the accuracy of the experiments of my adversaries, than to oppose them by arguments, or to allow due weight to their apparent consequences; and I thought it more honourable to acknowledge my conviction of their importance, than to persist either in error or in silence. I judged, with respect to the matter of fact, perhaps erroneously, but with regard to all the evidence that was then in existence, I judged as every unprejudiced mind must have been inclined to do. It was only in the year 1800 that I was induced to resume the investigation, in consequence of reading, in the Medical essays of a Society in Edinburgh, Dr. Porterfield's valuable paper "On the Internal Changes of the Eye." I improved on his ideas of the construction of an optometer, and I obtained, by numerous and diversified experiments, such accumulated evidence of the truth of my original opinion, that I was obliged to submit to the unexpected necessity of recurring to it once more. Those who have read my paper, not as a modern reviewer reads, but with patience and attention, will not, I imagine, think that any apology is required for this second change of sentiments. I cannot, however, refuse myself the pleasure of inserting here a passage from a letter which I have lately received from Dr. Olbers, the discoverer of the planet Pallas, the same whose dissertation on vision I have often quoted with applause. "You may easily suppose," says Dr. Olbers, "that your celebrated essay 'On the Mechanism of the Eye,'* must have interested me very particularly. I saw indeed that it completely refuted my own theory respecting the changes of the eye; but *my object is to discover truth, and not to support my opinion.*" With such a man as Dr. Olbers, my reviewer would say again, as he has said of me, it would be "difficult to argue: were we to

* No. II., p. 12.

take the trouble of refuting him, he might tell us, *My opinion is changed.*"

I have now, I trust, vindicated myself from the charge of any unwarrantable inconstancy in the changes which my opinions on the subject of vision have undergone. I shall next enter into a similar explanation of my motives for applying myself to the study of the phenomena of sound and light, and of the progress of my ideas respecting their nature. When I took a degree in physic at Göttingen, it was necessary, besides publishing a medical dissertation, to deliver a lecture upon some subject connected with medical studies: and I chose for this, the formation of the human voice. A few pages, containing a table of articulate sounds, were printed at the end of my dissertation "On the Preservative Powers of the Animal Economy:" my uncle, Dr. Brocklesby, at the instance of the late most respectable Dr. Heberden, repeatedly urged me to give some further explanation of the subject to which these characters related. When I began the outline of an essay on the human voice, I found myself at a loss for a perfect conception of what sound was, and during the three years that I passed at Emmanuel College, Cambridge, I collected all the information relating to it that I could procure from books, and I made a variety of original experiments on sounds of all kinds, and on the motions of fluids in general. In the course of these inquiries, I learned, to my surprise, how much further our neighbours on the continent were advanced in the investigation of the motions of sounding bodies and of elastic fluids, than any of our own countrymen: and in making some experiments on the production of sounds, I was so forcibly impressed with the resemblance of the phenomena that I saw, to those of the colours of thin plates, with which I was already acquainted, that I began to suspect the existence of a closer analogy between them than I could before have easily believed. On further reflection and examination my opinion was confirmed, and as I thought I could place the question in a clearer light than that in which it had generally been viewed, I was induced to insert my observations in a paper, which I presented soon after to the Royal Society, under the name of "Outlines of Experiments and Inquiries

respecting Sound and Light." * A determination to confine my studies as much as possible to physic was my motive for laying them before the Society in a state of confessed imperfection. I am not disposed to overrate their value; the compliment which was paid to them by an experienced veteran in philosophy, who wrote the best articles of the Encyclopædia Britannica, is fully as much as I can flatter myself that they deserve.† The motions of a stream of air, rendered visible by means of smoke, the diversified rotations of musical chords, the influence of the mode of agitation on the natural harmonics of strings, the phenomena of beats, and of grave harmonics, were examined in a manner which tended to place in a new point of view a subject certainly curious, and not wholly unimportant.

The opinion respecting light, which I first suggested in this paper as the most probable, was neither the same with Euler's, nor, as the reviewer falsely asserts, in any degree borrowed from him. It was precisely the theory of Hooke and of Huygens, with the adoption of some suggestions made by Newton himself as not in themselves improbable. The only objection which Newton makes to the hypothesis thus modified, is this:—light could not be propagated, solely by the undulations of a fluid, without spreading almost equally in all directions; and for this assertion he thinks that there is both experiment and demonstration. His arguments from experiment appear to me to have been sufficiently obviated by what Lambert has advanced in the Memoirs of Berlin, and by Professor Robison's remarks on echos in the Encyclopædia, as well as by many observations which I have myself made, at different times, on the waves of water. The demonstration is attempted in the Principia: to me it appears to be defective; if I am not allowed to be a competent judge, I can quote others, whose authority will not be denied. Euler has been called by some an indifferent philosopher, but he must at least be allowed to have been perfectly capable of judging of mathematical evidence: he had certainly read the Principia, and he utterly denied the conclusiveness of the argument. D'Alembert was a mathematician of acknowledged eminence, and Lalande's approbation of his sentiments must

* No. III., p. 64. † Supra, p. 134.

give them additional weight: both these mathematicians assert, as it appears from Lalande's edition of Montucla, that the arguments are so balanced in favour of the different systems of light, that the safest way is to confess "our utter ignorance of the manner of its propagation." The celebrated Laplace, in comparing the opinions respecting light, is contented to call the Newtonian doctrine a hypothesis, which, on account of the facility of its application to the phenomena, is extremely probable. If he had considered the undulatory system as demonstrably absurd, he certainly would not have expressed himself in so undecided a manner. The opinion of Franklin adds perhaps little weight to a mathematical question, but it may tend to assist in lessening the repugnance which every true philosopher must feel, to the necessity of embracing a physical theory different from that of Newton.

I have indeed been accused of insinuating "that Sir Isaac Newton was but a sorry philosopher." But it is impossible that an impartial person should read my essays on the subject of light without being sensible that I have as high a respect for his unparalleled talents and acquirements as the blindest of his followers, and the most parasitical of his defenders. I have acknowledged that "his merits are great beyond all contest or comparison;" that "his discovery of the composition of white light would alone have immortalised his name;" that the very arguments which tend to overthrow his hypothesis respecting the emanation of light, "give the strongest proofs of the admirable accuracy of his experiments;" and that a person may, "with the greatest justice, be attached to every doctrine which is stamped with the Newtonian approbation." The printer of the Review, feeling perhaps that the last expressions would militate too much in my favour, has thought fit to plunder me of them, by omitting the marks of quotation, and to attribute them to my antagonist. But, much as I venerate the name of Newton, I am not therefore obliged to believe that he was infallible. I see, not with exultation, but with regret, that he was liable to err, and that his authority has, perhaps, sometimes even retarded the progress of science. It is now no longer denied that he was mistaken in an optical experiment respecting

the dispersion of light; and the only attempt that has been made to explain the mistake merely shows that there was a possibility of his being misled by a singular combination of circumstances: in a case of mathematical optics he was certainly mistaken, as Dr. Smith has shown, when he asserted that a sphere of water produces a maximum of density in the light refracted at an angle of about 26°: in the mechanical estimation of force he erred when he calculated the precession of the equinoxes, and estimated the rotatory power of each particle of the earth's substance as simply proportional to its distance from the axis. These mistakes, and perhaps some others, have been acknowledged and corrected by later writers: other persons, less considerate, have attacked him where he was invulnerable. One of these is the gentleman whom I have reason to think the author of the remarks to which I am replying, and who, having first accused Newton of a palpable and fundamental blunder, appears now to be desirous of securing to himself the exclusive privilege of questioning his authority.

What I have hitherto said relates to the state of the question respecting the nature of light, as it stood before the publication of the first of the papers which have excited so much virulence. But I assert that this paper contains an argument, sufficient to convert that which before was doubt and conjecture, into probability and conviction. It was in May 1801 that I discovered, by reflecting on the beautiful experiments of Newton, a law which appears to me to account for a greater variety of interesting phenomena than any other optical principle that has yet been made known. I shall endeavour to explain this law by a comparison.

Suppose a number of equal waves of water to move upon the surface of a stagnant lake, with a certain constant velocity, and to enter a narrow channel leading out of the lake. Suppose then another similar cause to have excited another equal series of waves, which arrive at the same channel, with the same velocity, and at the same time with the first. Neither series of waves will destroy the other, but their effects will be combined: if they enter the channel in such a manner that the elevations of one series coincide with those of the other, they must together

produce a series of greater joint elevations; but if the elevations of one series are so situated as to correspond to the depressions of the other, they must exactly fill up those depressions, and the surface of the water must remain smooth; at least I can discover no alternative, either from theory or from experiment.

Now I maintain that similar effects take place whenever two portions of light are thus mixed; and this I call the general law of the interference of light. I have shown that this law agrees, most accurately, with the measures recorded in Newton's Optics, relative to the colours of transparent substances, observed under circumstances which had never before been subjected to calculation, and with a great diversity of other experiments never before explained. This, I assert, is a most powerful argument in favour of the theory which I had before revived: there was nothing that could have led to it in any author with whom I am acquainted, except some imperfect hints in those inexhaustible but neglected mines of nascent inventions, the works of the great Dr. Robert Hooke, which had never occurred to me at the time that I discovered the law; and except the Newtonian explanation of the combinations of tides in the Port of Batsha.

It is unnecessary, on this occasion, to enter minutely into the consequences of the law of the interference of light: they have been the principal subjects of the three papers which have drawn down upon me the repeated anathemas of the self-erected Inquisition of the North. Not a single argument has been produced to invalidate it. The reviewer has cursorily observed that if the law were true, every surface opposed to the light of two candles would appear to be covered with fringes of colours. Let us suppose the assertion true—what will be the consequence? In all common cases the fringes will demonstrably be invisible; since, if we calculate the length and breadth of each fringe, we shall find that a hundred such fringes would not cover the point of a needle; and an optician does not require to be told that a mixture like this constitutes a white light, not distinguishable by the senses from that which is supposed to have formed them.

In order to answer the charge of inconsistency in my opinions

respecting the nature of light, I must begin by observing that there are two general methods of communicating knowledge; the one analytical, where we proceed from the examination of effects to the investigation of causes; the other synthetical, where we first lay down the causes, and deduce from them the particular effects. In the synthetical manner of explaining a new theory we necessarily begin by assuming principles, which ought, in such a case, to bear the modest name of hypotheses; and when we have compared their consequences with all the phenomena, and have shown that the agreement is perfect, we may justly change the temporary term *hypothesis* into *theory*. This mode of reasoning is sufficient to attach a value and importance to our theory, but it is not fully decisive with respect to its exclusive truth, since it has not been proved that no other hypothesis will agree with the facts.

It is exactly in this manner that I have endeavoured to proceed in my researches. By analysing the experiments of Newton, and comparing them with my own, I had arrived at principles, to which I gave, in my paper on the theory of light, the unassuming title of hypotheses; after comparing these principles with all the phenomena of light, and showing their perfect consistency, I thought myself authorised to make a conclusion, in my ninth proposition, which converts the hypothesis into a theory. I was justified in doing this, because no man had ever attempted to advance a theory which would bear to be compared mathematically with the phenomena that I enumerated. But, according to the nature of the only mode of reasoning which the circumstances allowed me, it was impossible to infer, from this synthetical comparison, that no other suppositions would agree with the phenomena; and *I expressly remarked*, with respect to one of the four hypotheses which I laid down, that it was possible to find *others which might be substituted for it*. It is in this hypothesis and its consequences only, that I have since attempted to make any improvements. And such improvements I shall ever admit with pleasure, whether they arise from my own experiments, or from those of others. One immaterial correction of this kind I was obliged to make in consequence of Dr. Wollaston's most interesting observations upon the true

division of the prismatic spectrum, which afford an additional proof that even Newton's experiments, frequently as they have been repeated by others, may sometimes stand in need of a more careful examination. And this modification, which has, in fact, little or no connexion with the essential parts of my theory, has been adduced as a proof of the "fickle and vibratory nature of the medium that fills my mind." The reviewer has indeed in another place denied the accuracy of Dr. Wollaston's experiment, but his objections are too futile to deserve notice.*

Respecting another trifling change of sentiment, to which the reviewer has thought proper to attach great importance, I have hitherto abstained from explanation, in delicacy towards the gentleman whose observations were concerned; I wish to avoid insisting on his inaccuracy in a very easy calculation; and for the same reason, I shall say nothing further on the subject at present.

When the reviewer asserts that "a hypothesis is a work of fancy, useless in science," it must be supposed that he is speaking of such hypotheses as have neither been originally deduced from experiments, nor afterwards compared with them: but when, in another of his articles, he condemns, as having impeded

* In the following notice in the Edinburgh Review for April, 1803, of his paper in the Philosophical Transactions for 1802, 'On the oblique Reflection of Iceland Crystal.'—"We were much disappointed to find, that so acute and ingenious an experimentalist had adopted the wild optical theory of vibrations. After stating it, however, chiefly from Huygens, and applying it to explain the properties of the spar, he goes on to examine, by accurate experiments, whether the undulatory system agrees with the facts. The hypothesis is, that the different undulations of the elastic medium are spherical in almost all cases, but that, in the Iceland crystal, those undulations are spheroidal; and it must be acknowledged, the near coincidence of the experiments, which are extremely well contrived, and appear to be accurately conducted, give this theory a plausibility which it did not before possess. We would, however, remark, that the hypothesis of Aepinus himself, by far the most consistent, simple, and universally applicable, of any that has ever been proposed, is still only a gratuitous hypothesis; has acquired to its author only the praise of fanciful ingenuity; and has perhaps done more harm than good to the science of magnetism, by withdrawing the attention of philosophers from the patient and difficult, but profitable observation of nature, to the more easy but empty amusement of indulging their fancy.

"The hypothesis of Huygens is not, as Dr. Wollaston seems to think, the same with that of Euler and other unphilosophical inquirers. It approaches more nearly to that of Newton, and assumes the existence of an elastic medium, acting upon and influenced by the rays of light. These authors, misled by the nature of sound, do not admit the materiality of light, but assert that it is a vibration propagated through the medium. But, short as these remarks are, we are loth to waste any more time on such a feeble and ill-conducted defence of an untenable and useless hypothesis." Vol. ii. p. 99.—*Note by the Editor.*

the progress of discovery, the beautiful hypothesis which has been applied, with the greatest success, by Æpinus, by Mr. Cavendish, and by Professor Robison, to the phenomena of electricity and magnetism; we can only regret that a person so void of a sense of physical elegance should have an opportunity of obtruding opinions like these on the public; and we may expect that he would say, if he dared, that even the hypothesis of universal gravitation has presented an insuperable barrier to the advancement of experimental knowledge. He is at least determined to show that every hypothesis must be the work either of infancy or of dotage; and insinuates that the speculations which I have extracted from Newton's writings were merely the amusements of some vacant hours at the close of his scientific career. It is very true that the *queries* of Newton were given "to the world" at a time when his brilliant and solid discoveries were fully established; but the papers which explain all his hypotheses concerning light the most at large, and to which I have had the most frequent occasion to refer, were read *to the Royal Society* more than ten years before he began to write his Principia; and the principal reason that delayed their publication, appears to have been the apprehension of disputes with Dr. Hooke. Some were published in the Optics, soon after Dr. Hooke's death; others are only to be found in Birch's History of the Royal Society. Had I not taken care to annex the dates to my quotations, the reviewer might easily have pleaded his ignorance in excuse for his misrepresentations.

The same plea of ignorance would be but an inadequate apology for the assertion of a positive falsehood, where he accuses me of referring to an *unpublished* work of my own. The reference could only be intended for the readers of the essay as a printed paper; my Syllabus was published in January, 1802; the Transactions not till late in the spring; and if he had either sent to the publisher for this syllabus, or made inquiry for it among his literary friends even in Edinburgh, he might have found in it some information, on subjects which he appears to understand but imperfectly.

In the first paragraph of the review of my paper on the production of colours, the writer confesses that he has not "*suffi-*

cient fancy to discover" how the "interference of two portions of light" could ever produce an appearance of colour. The poverty of his fancy may indeed easily be admitted, but it is unfortunate that he either has not patience enough to read, or intellect enough to understand, the very papers that he is criticising; for, if he had perused with common attention my Bakerian lecture on light, he might have understood such a production of colour without any exertion of fancy at all. He then quotes from me the assertion, that a "black hair" does not produce the appearance of fringes, and he has even the modesty to refer to a certain page of my paper. I have there said, that a "*horse* hair" did not produce that appearance; and I have left it for the reviewer to decide whether the *horse* should be white or black. The truth is, that a fine wire, or a small hair, whether black or white, exhibits equally well the colours which I have described. If the fact were otherwise, it would be utterly unintelligible; for there is absolutely no foundation for the reviewer's insinuation, that any theory of these colours was deduced by De Dominis, or can be deduced by any other person, from the laws of refraction. He asserts that it is mathematically impossible for the light to bend round a hair; Grimaldi has long ago experimentally demonstrated this flexion, and called it diffraction; an effect which furnishes the most striking analogy between the motions of light and those of the waves of water.

The reviewer next complains of his utter *want of comprehension* of the difference between the colours of mixed plates, and those of the plates which have been described by Newton. Had he sufficiently studied the Optics of Newton, he would have seen that the thickness of a simple plate of water must be only *three fourths* as great as that of a plate of air, in order to produce similar effects: in the colours which I have described, the thickness of the mixed plate was *six times* as great as that of the plate of air: the one series of rings *expanded*, upon inclining the plates, the other *contracted*. These distinctions are plain enough for any person of *ordinary* comprehension; and I was not aware that it was necessary to provide for *extraordinary* cases.

We are induced to suppose, from the page which immediately

follows, that, to speak without a metaphor, neither the fancy nor the comprehension of the reviewer could enable him to distinguish a black spot from a white one. I have said, that when two glasses are brought into the most intimate contact possible, with the interposition of a certain fluid, the central spot of the rings of colours is nearly white: it was before known that, without any such interposition, the central spot, in similar circumstances, would be nearly black: and the critic sagaciously pronounces, that these effects are precisely the same. He quotes from Newton the expression of the "pellucid central spot," meaning the spot which reflected no light, and then explains it, as if it were exactly similar to that which, in my experiment, reflected nearly all the light that fell on it, and was therefore white.

That the lines which are quoted in the same page, from my paper, present, when thus insulated, an appearance of confusion and of vague reasoning, is perhaps undeniable, and is perfectly excusable. The reviewer has not understood the paper in its entire state, and he might be sufficiently secure, that his readers would never be able to extricate an intelligible sense from an arbitrary quotation of a few lines, taken out of the middle of a paragraph of connected reasoning. He misapprehends and misrepresents completely the whole subject of the explanation; he says that its object is to explain the blue colour of the lower part of the flame of a candle. Nothing was further from my thoughts than to assign any reason for this blueness: what I attempted to illustrate, was an original and important observation made by Dr. Wollaston, that a portion of the blue flame of a candle appeared, when viewed though a prism, to be divided into a number of distinct masses or images. My illustration of this phenomenon has not the slightest connexion with what the reviewer calls his solution of the appearance of different colours in different flames, which he so humbly intreats his readers to compare with it. I am not therefore obliged to give an opinion of any kind respecting this pretended explanation of a phenomenon foreign to the subject; if I were, it would be sufficient to say, that no such laws could be supposed to operate, upon the principles of mechanical forces, without producing different

velocities in light of different colours. But the passage fortunately affords me a most convincing proof of the nature of the source from which this torrent of invective has originated. We are here told, that the doctrine of the different *flexibility* of light is now *universally admitted*. I have searched into all the works that I could find in the libraries to which I have had access, for opinions respecting the nature of light, and, as far as I have discovered, the different flexibility of light is *admitted*, in the absurd and unwarrantable sense in which it is here employed, *by three writers only*. The first is Mr. Henry Brougham, the second the anonymous author of an article in the Encyclopædia Britannica, and the third the assailant whose injurious attacks I am now repelling. From so remarkable a coincidence, I think myself authorised to conclude, that these three writers are one and the same. I have before hinted that Mr. Brougham's doctrines have been sufficiently confuted, by Professor Prevost of Geneva.* Mr. Prevost has satisfactorily defended the experiments of Newton from the imputations of Mr. Brougham; but in other respects he has perhaps treated the young theorist with too much lenity.

I have now answered everything that was intended as an argument, in the articles published in the second number of the Review. This constitutes, in fact, but a small part of those articles: they have much less the appearance of the impartial discussion of a long disputed question in natural philosophy, than of the buffoonery of a theatrical entertainment, or of the jests of a pert advocate, endeavouring to place in a ridiculous light the evidence of his adversary. To answer such an attack in similar language would be degrading; to attempt to oppose it by argument would be futile. I shall refrain, therefore, from noticing any of the additional scurrilities which have been copiously intermixed by the same writer with his remarks on my last paper. I say the same, because I am unwilling to suppose that this island has produced two persons capable of so stupidly misunderstanding, and so wilfully misrepresenting.

* In the Philosophical Transactions for 1798, vol. lxxxviii., p. 321, in a paper entitled 'Some Optical Remarks chiefly relative to the Reflexibility of the Rays of Light.'—*Note by the Editor.*

But their identity is of no consequence to the discussion, and it is unnecessary to inquire for proofs of it. The whole purpose of the paper inserted in the ninth number of the Review might be supposed to have been, not to confute the principles which the writer attacks, but to show that he is incapable of understanding even the simplest of them.

I have asserted, that two series of *undulations*, *interfering* with each other at certain relative *intervals*, necessarily produce certain modifications in their joint effects. These terms not only belong to the same theory, but are parts of the same position which I have already illustrated by a familiar comparison in these remarks. The author of the critique has sagaciously observed, that "they who object to the theory of interference, have only to *turn a page*, and they find the theory of intervals, and they need but go on *a section further*, and the vibrations and undulations are very much at their service."

This specimen is sufficient to explain how naturally it must appear to him "unaccountable," that the process of interference should produce certain effects, some of which I never supposed that it could produce, and others which none who rightly understood my theory could ever doubt that it must produce. He asks, "on what known principle" can the production of coloured fringes from two beams of white light be explained? I answer, certainly on no principle that was known before; but upon consideration of the law which I have discovered, most simply and unavoidably.

The reviewer has afforded me, in the next observation, an opportunity for a triumph as gratifying as any triumph can be where the enemy is so contemptible. Conscious of inability to explain the experiment which I have advanced, too ungenerous to confess that inability, and too idle to repeat the experiment, he is compelled to advance the supposition that it was incorrect, and to insinuate that my hand may easily have erred through a space so narrow as one-thirtieth of an inch. But the truth is, that my hand was not concerned: the screen was placed on a table, and moved mechanically forwards with the utmost caution; the experiment succeeded in some circumstances where the breadth of the object was doubled or tripled; and I assert

that it was as easy to me to estimate an interval of one-thirtieth of an inch, as an interval a hundred or a thousand times as great. Let him make the experiment, and then deny the result if he can.

With equal pertinacity of blundering, he has remarked that the interference of light inflected by two contiguous edges, ought, upon my principles, to produce, not continued fringes, but only "square or rectangular spots of fringe." Was it not enough to have demonstrated the weakness of his powers with regard to physical laws? And was it necessary to induce his readers to suppose him incapable of going through a little algebraical calculation leading to the properties of the hyperbola? Let right lines be inflected from the edges of a rectangular object into its shadow, so as to cut off portions from the opposite lines, exceeding their own length by a given interval, and I maintain that the intersections will form continued curves, and that those curves will be hyperbolas: the shape of the fringes ought *not*, therefore, to be that of detached spots, but of hyperbolical curves.

It is "a metaphysical absurdity," says the reviewer, to assert that qualities can "move" in concentric surfaces. I have not said that the qualities of light "move" in concentric surfaces, but that they "succeed each other" in concentric surfaces; and in this there is certainly no metaphysical absurdity. Condensation and rarefaction are qualities of the air, and it will not be denied that, in every musical sound, condensation and rarefaction continually succeed each other in concentric surfaces.

Upon my train of argument respecting the nature of light, the reviewer observes, first, that an analogy is made the ground of an inference. I answer, that when the analogy is sufficiently close, it is a most satisfactory ground of physical inference. Secondly, he says that a gratuitous assumption is set down as a necessary truth. I reply, that the assumption is not gratuitous; that nobody, except for the sake of argument, will deny, or can deny it; should it be denied, it would be perfectly easy to substantiate it by showing the unavoidable contradictions that would result from any alternative that could be substituted for it. The remaining part of the paragraph is as correctly quoted

as that edition of the Bible was printed, in which the only error was the omission of the word *not* in the seventh Commandment: here the monosyllable *but*, which completely inverts the sense of the passage, and which would have entirely destroyed the force of the criticism, is therefore very prudently omitted.

I have inserted a caution relating to deceptions in the examination of microscopical objects, not in order to attach any additional merit to my own explanations, but as a hint naturally arising out of the subject. The same caution might perhaps have been suggested by the results of some former experiments, but the particular appearances that would be produced by such fallacies could never before have been so minutely indicated. That the images of very small objects on the retina may possibly be affected by such causes, is the natural inference from my principles; and it is of no consequence to this position whether the reviewer can or cannot explain them from his own.

My comparison of a grove of trees pervaded by the wind, with the particles of a material body, separated, as all modern philosophers have supposed them to be, by intervals incomparably greater than their diameters, and allowing an inconceivably rare medium to penetrate with perfect freedom every interstice, could scarcely have appeared obscure or inapplicable to any man unblinded by prejudice or unbiassed by malevolence.

I have already said enough of Newton to show how I venerate his character, as the first of mathematicians and the greatest of philosophers. Perhaps, however, the mention of persons whose views are "still less enlarged" than his own, may imply in some measure what I never intended, and may therefore require some little apology, especially as the expressions will bear to be applied to the objections which I am now endeavouring to refute. It was, indeed, a want of respect to his illustrious memory to place the superficial and dogmatical fancies of a writer in the Edinburgh Review in any kind of comparison with the deep and refined imaginations of a Newton. Instead of "still less enlarged" and enlightened, I

ought to have called them narrow and confused, selfish and interested, puerile and ostentatious.

The indignation of the same violent and arbitrary tribunal has been excited and called forth by a declaration from a man whose approbation is so much the more valuable as it is always bestowed with the most cautious regard to experimental accuracy and logical induction. Dr. Wollaston has observed that "the theory of Huygens affords, as has lately been shown by Dr. Young, *a simple explanation of several phenomena not yet accounted for by any other hypothesis.*" His own observations on Iceland crystal accord throughout, he says, with this hypothesis of Huygens; the measures that he has taken "correspond more nearly than could well happen to a false theory." But he contents himself with stating these undeniable facts; and the reviewer goes too far when he asserts that Dr. Wollaston "has adopted the wild optical theory of vibrations." If Dr. Wollaston had then been acquainted with the experiments and calculations which I have made since that time, it is possible that his assent might have been much more complete and unreserved. But while I allow to his experiments all the merit that a clear conception, a vigorous mind, a steady hand, and an accurate eye can bestow on them, it must not be said by the Edinburgh Reviewers that his experiments have given the theory "a plausibility which it did not before possess." As experiments, they have all the merit of originality, for the author, when he made them, was unacquainted with those of Huygens; and his most ingenious invention of an instrument for measuring refractive powers enabled him, with great ease, to improve and extend them. But the experiments of Huygens were elaborate and diversified, and every argument that can be inferred from Dr. Wollaston's observations had been anticipated by this great philosopher upon the ground of his own. It is true that our reviewer was not likely to have troubled himself with Huygens's treatise of light; his business is to censure others, and not to inform himself; it was easier for him to call this doctrine "a clumsy hypothesis," and "a dull invention," than to investigate its truth, and to admire its elegance. He has indeed made distinctions between Huygens's doctrine and mine, which serve

but to prove still more strongly that he was acquainted with neither; I shall only answer his epithets by a quotation from a writer, whose merits the testimony of Newton is well known to have raised far above the ordinary rank of his contemporaries. In Cotes's lectures on hydrostatics, where he is speaking of the velocity of light, and takes occasion to mention the hypothesis of Huygens, the following passage occurs:—"When we take a particular view of the several parts of this hypothesis, it appears to be so very ingeniously contrived, and so handsomely put together, that one can hardly forbear to wish it were true." The evidence was at that time imperfect, but the symmetry was complete.

The reviewer has thought proper to unite, in several instances, with his invectives against me, some ridicule of the objects of the Royal Institution of Great Britain; an institution in which its Managers have studied to concentrate all that is useful in science, or elegant in literature. This connexion appears to him to add so much weight to his arguments, that he has chosen, without further provocation, to insinuate its existence more than a year after it had been dissolved. I accepted the appointment of Professor of Natural Philosophy in the Royal Institution as an occupation which would fill up agreeably and advantageously such leisure hours as a young practitioner of physic must expect to be left free from professional cares. I was led to hope that I should be able to impress an audience formed of the most respectable inhabitants of the metropolis, with such a partiality as the moderately well-informed are inclined to entertain, for those who appear to know even a little more than themselves of matters of science; that I might be of use to the public in disseminating the true principles of natural philosophy; and that I might in future be remunerated by the enjoyment of a more extensive confidence in my professional abilities than could have been granted to a person less generally known. While I held the situation, I wished to make my lectures as intelligible as the nature of the subjects permitted; but I must confess that it was not my ambition to render them a substitute for those of any superficial experimenter, that was in the habit of delivering courses of natural philosophy for the

amusement of boarding schools. Whatever may have been the imperfections of my lectures, it cannot be asserted, except perhaps in the Edinburgh Review, that they were fit for audiences of ladies of fashion only. After fulfilling, for two years, the duties of the Professorship, I found them so incompatible with the pursuits of a practical physician, that, in compliance with the advice of my friends, I gave notice of my wish to resign the office. I think it, however, just to the Institution, to the public, and to myself, that the result of my labours, throughout the whole extent of natural philosophy and the mechanical arts, should be rendered of some permanent utility; and I have since collected such a mass of further references to works of all ages and of all nations, accompanied by many notes and extracts from them, that it will henceforwards be easy for every student and every author to know at once what has been done, and what remains to be done, in the subject of his particular researches; and to what books he must apply for the best information; where further information is required, and can be obtained. Considering how widely this information is at present scattered, I trust that I shall have rendered a service of some importance to every department of the sciences, and I am now on the point of preparing my book for immediate publication. With this work my pursuit of general science will terminate: henceforwards I have resolved to confine my studies and my pen to medical subjects only. For the talents which God has not given me, I am not responsible, but those which I possess, I have hitherto cultivated and employed as diligently as my opportunities have allowed me to do; and I shall continue to apply them with assiduity, and in tranquillity, to that profession which has constantly been the ultimate object of all my labours.*

Welbeck Street, 30th Nov. 1804.

* Of the preceding most masterly Reply, which was published in the form of a pamphlet, it was stated by its author, that *one copy only was sold*: it consequently produced no effect in vindicating his scientific character, or in turning the current of public opinion in favour of his theory.—*Note by the Editor.*

No. XI.

AN ACCOUNT OF DR. YOUNG'S

HARMONIC SLIDERS.

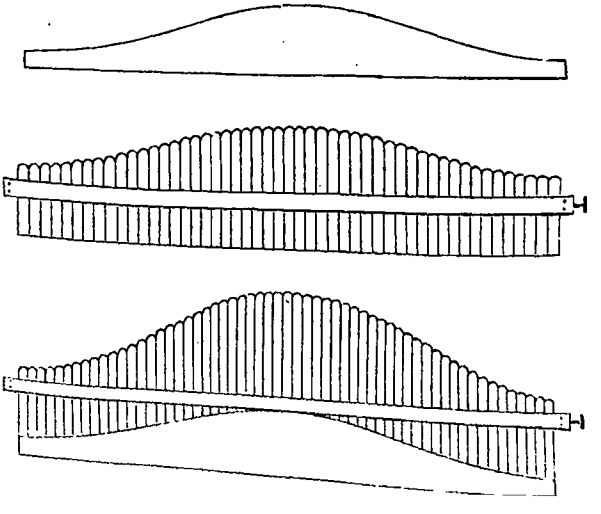
From the Journals of the Royal Institution of Great Britain for 1802, vol. i., p. 261.

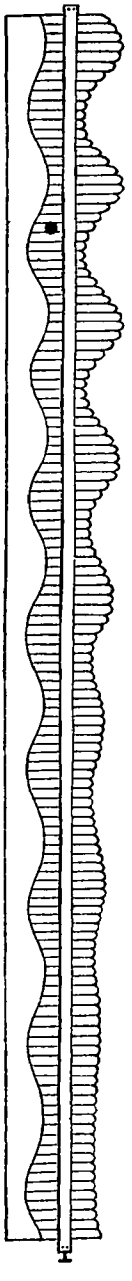
THE combination of undulations, however cautiously the world may adopt its application to the explanation of optical phenomena, is of acknowledged utility in illustrating the phenomena of musical consonances and dissonances, and of undeniable importance in accounting for many of the phenomena of the tides. Each tide is an undulation on a large scale; and, supposing the general form of the ocean, in consequence of the attraction of a distant body, to coincide with that of an oblong spheroid, as it is found by calculation to do, the section of the surface of each tide, if conceived to be unbent from the circular form and extended on a plane, would form the harmonic curve. (Young's Syllabus IV. 151. 155.) It is remarkable that the motions of the particles of the air in sound, have been generally supposed in theory to correspond with the ordinates of this same curve, and that there is also experimental reason to believe, that the purest and most homogeneous sounds do in fact agree very nearly with the law of this curve. It is therefore by far the most natural as well as the most convenient to be assumed, as representing the state of an undulation in general; and the name of these harmonic sliders is very properly deduced from the harmonic curve.

By means of this instrument, the process of nature, in the combinations of motion which take place in various cases of the

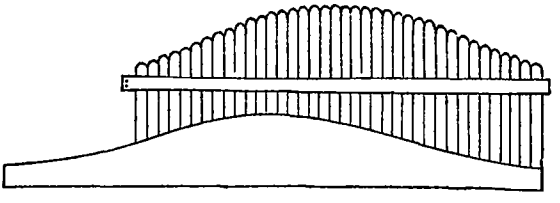
junction of undulations, is rendered visible and intelligible, with great ease, in the most complicated cases. It is unnecessary to explain here, how accurately both the situations and motions of the particles of air, in sound, may be represented by the ordinates of the curve at different points; it is sufficient to consider them as merely indicating the height of the water constituting a tide, or a wave of any kind, which exists at once in its whole extent, and of which each point passes also in succession through any given place of observation. We have then to examine what will be the effect of two tides, produced by different causes, when united. In order to represent this effect, we must add to the elevations or depressions in consequence of the first tide, the elevations or depressions in consequence of the second, and subtract them when they counteract the effect of the first: or we may add the whole height of the second above any given point or line, and then subtract, from all the sums, the distance of the point assumed below the medium.

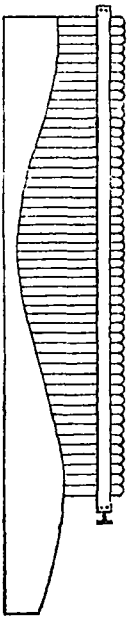
To do this mechanically is the object of the harmonic sliders. The surface of the first tide is represented by the curvilinear termination of a single board. The second tide is also represented by the termination of another surface; but, in order that the height at each point may be added to the height of the first tide, the surface is cut transversely into a number of sepa-





rate pieces or sliders, which are confined within a groove or frame, and tightened by a screw. Their lower ends are situated originally in a right line; but, by loosening the screw and moving the sliders, they may be made to assume any other form: thus they may be applied to the surface representing the first tide; and, if the similar parts of each correspond, the combination will represent a tide of twice the magnitude of the simple tides. The more the corresponding





parts are separated, the weaker will be the joint effect; and, when they are furthest removed, the whole tides, if equal, will be annihilated. Thus, when the general tide of the ocean arrives by two different channels at the same port, at such intervals of time that the high water of one would happen at the same instant with the low water of the other, the whole effect is destroyed, except so far as the partial tides differ in magnitude. The principle being once understood, it may easily be applied to a multiplicity of cases: for instance, where the undulations differ in their dimensions with regard to extent. Thus, the series of sliders being extended to three or four alternations, the effect of combining undulations in the ratio of 2 to 1, of 3 to 1, of 2 to 3, of 3 to 4, may be ascertained, by making a fixed surface,

terminating in a series of curves, that bear to those of the sliding surface the ratio required: and, by making them differ but slightly, the phenomenon of the beating of an imperfect unison in music may be imitated, where the joint undulation becomes alternately redoubled and evanescent. In the last figure here inserted, the proportion is that of 17 to 18, and the curvilinear outline represents the progress of the joint sound from the greatest degree of intensity to the least, and a little beyond it.

There is no conception more difficult, and few more important, than that of the transmission of waves and the effects of their interference with each other. The contrivance explained in the text exhibits those effects in waves of water and air, and even of light, assuming that the vibrations which produce them are in the direction of the wave's motion. Other contrivances, somewhat analogous to it, have been made by the present Astronomer Royal and Mr. Wheatstone, which exhibit the movements and interferences of waves upon any hypothesis of vibration, which the phenomena of light, more especially those of its polarization and transmission through crystallized media, may render necessary.—*Note by the Editor.*

No. XII.

REVIEW OF LAPLACE'S MEMOIR "SUR LA LOI DE

LA RÉFRACTION EXTRAORDINAIRE DANS LES CRISTAUX DIAPHANES.

Lu à la première Classe de l'Institut, dans sa séance du 30 *Janv.* 1809.

Journal de Physique, Janv. 1809."

From the Quarterly Review for Nov., 1809, vol. ii., p. 337.

THE few who have arrived, in the different departments of learning and science, at such a degree of eminence as to be almost "without a second, and without a judge," have not only the advantage of being able to propagate real knowledge with uncontrolled authority, but also the less enviable prerogative of giving to error the semblance of truth, whenever accidental haste or inattention may have led them into those inaccuracies, from which no human intelligence can be wholly exempt.* It is necessary, therefore, for a critic, who undertakes to make a faithful report of the progressive advancement of the sciences, to watch with redoubled care the steps of those, who are the most likely to lead others astray, if they happen to follow a wrong path: and while the ultimate decision always remains with the public, as with a jury, the judge is bound to state, as fully

* Sir David Brewster, in a letter to Dr. Young, dated Janunry, 1818, mentions a very striking example of the implicit deference paid to the authority of this great philosopher by one of the most distinguished of his followers.—". . . . . did not scruple to declare, when I saw him in Edinburgh last summer, that any experimental laws of double refraction which I might have discovered *must* be erroneous, unless they agreed with those given by Laplace; and that I ought to compare them with his before I published them. My reply was, that as my laws were deduced from experiment, it was rather Laplace's affair to see that his theoretical ones agreed with mine." It is proper to add, however, that the confidence formerly reposed in the correctness of Sir Isaac Newton's optical theories and experiments was almost equally unconditional, and still more injurious to the progress of the science.—*Note by the Editor.*

and impartially as possible, the whole mass of the evidence before him; not fearing to adduce all such reasoning as can tend to the support of the weaker side, when there is any danger of its being oppressed by the authority and respectability of the stronger.

These reflections have been suggested to us by an essay, for which we are indebted to a very celebrated continental mathematician—a man of whom we willingly say, with Heraclitus, εἷς ἐμοὶ ἄνθρωπος τρισμύριοι, but on whose works we thought it necessary, on a former occasion, to make some free remarks. We then objected to him a want of address or of perseverance in the management of his calculations, presuming that the principles, on which they were founded, were capable of being applied, with much greater precision, to the phenomena in question: our suspicion has since that time been justified by an essay of an anonymous author in this country, who, without any great parade of calculation, appears to have afforded us a general and complete solution of the problem, which Mr. Laplace had examined in particular cases only. We have now to accuse him of an offence of a different complexion—that is, the hasty adoption of a general law, without sufficient evidence; and an inversion of the method of induction equally unwarrantable with any of the paralogisms of the Aristotelian school. We complain also, on national grounds, of an unjustifiable want of candour, in not allotting to the observations of different authors their proper share of originality. What has a man of science to expect from the public, as a reward for his labours, but the satisfaction of having it acknowledged, that he has done something of importance towards extending the sphere of intellectual acquirements? And who is so capable of directing public opinion, on subjects respecting which very few will form an opinion of their own, as a philosopher like Mr. Laplace, whose works are sure of commanding universal attention, and almost sure of inforcing implicit belief? The Huygenian law, of the extraordinary refraction of Iceland crystal, has lately, he says, been confirmed by "Mr. Malus." We know nothing of the extent of Mr. Malus's researches, but we know that Mr. Laplace sometimes reads the Philosophical Transactions, and

he either must have seen, or ought to have seen, a paper published in them by Dr. Wollaston, as long ago as the year 1802, which completely establishes the truth of the law in question, on the most unexceptionable evidence, and by the most accurate experiments. But it seems to be one of the attributes of a great nation to disregard, on all convenient occasions, the rights of its neighbours; we might have made the same remark in our former criticism * on Mr. Laplace, but there is so little novelty in the circumstance, that it is unnecessary to dwell any further on it at present.

It has long been known to opticians, that many crystals, of different kinds, have the remarkable property of making substances, viewed through them in certain directions, appear double; the effect of their refraction being almost the same as if a rarer and a denser medium existed together in the same space; some part of the light passing through them being refracted in the same manner as if the denser medium alone were present, and some as if the rarer only were concerned. The reason of this double refraction is wholly unknown, nor has any attempt been hitherto made to discover it. The crystals of carbonate of lime, in their primitive form, have also a further peculiarity: they afford a double image, even when the object is viewed perpendicularly through the two opposite and parallel sides of a crystal; an effect which could never arise from the combination of any two mediums acting in the ordinary manner; in fact, one of the images only is seen according to the laws of ordinary refraction, and the place of the other is determined by a law, which is the subject of the present paper. This law was experimentally demonstrated, and very elegantly applied to the phenomena by its first discoverer Huygens; but having been suggested to him by an hypothesis which was not universally adopted, it was rejected or neglected by his antagonists, without any accurate investigation; and the testimony of the greatest philosophers of that age, or of any age, having been opposed to it, it remained forgotten for almost a century. Nor is this the only instance in which, even within the limits of the physical

* Quarterly Review for February, 1809, vol. i., p. 107. *Théorie de l'Action Capillaire*, par M. Laplace.—*Note by the Editor.*

sciences, high authority has been suffered to prevail against unassuming truth. Mr. Haüy is the first of the later observers, who remarked that the true law of extraordinary refraction was much nearer to the Huygenian law, than to that which had been substituted for it by Newton. Some time afterwards, Dr. Wollaston had made a number of very accurate experiments, with an apparatus singularly well calculated to examine the phenomena; but he could find no general principle to connect them, until the work of Huygens was pointed out to him: he was then enabled, by means of the Huygenian law, to reduce his experiments to a comparison with each other; and in communicating them to the Royal Society, he remarked that "the oblique refraction, when considered alone, seemed to be nearly as well explained as any other optical phenomena." Here the matter rested, until Mr. Malus made the experiments which have led to the present paper.

"Mr. Malus has lately compared the Huygenian law," says Mr. Laplace, "with a very great number of experiments, made with extreme precision, on the natural and artificial surfaces of the crystal, and has found that the law agrees exactly with his experiments, so that it must be placed among the most certain, as well as among the most striking results of physical observation. Huygens had deduced it, in a very ingenious manner, from his hypothesis respecting the propagation of light, which he imagined to consist in the undulations of an ethereal fluid. This great geometrician supposed the velocity of the undulations in the ordinary transparent mediums to be smaller than in a vacuum, and to be equal in every direction: in the Iceland crystal, he imagined two distinct species of undulations; the velocity of the one being the same in all directions, in the other variable, and represented by the radii of an elliptic spheroid, having the point of incidence for its centre, and its axis being parallel to that of the crystal; that is, to the right line which joins the two obtuse solid angles of the rhomboid. Huygens does not assign any cause for this variety of undulations; and the singular phenomena, exhibited by the light which passes from one portion of the crystal into another, are *inexplicable* upon this hypothesis. This circumstance, together with the great difficulties presented by the undulatory theory of light in general, has induced the greater number of natural philosophers to reject the law of refraction founded on the Huygenian system. But since experiments have demonstrated the accuracy of this remarkable law, it must be entirely separated from the hypothesis

which originally led to its discovery. It would be extremely interesting to reduce it, as Newton has reduced the law of ordinary refraction, to the action of attractive or repulsive forces, of which the effects are only sensible at insensible distances; it is indeed very probable that it depends on such an action, as *I have satisfied myself* by the following considerations.

"It is well known that the principle of the least possible action takes place, *in general*, with respect to the motion of a material point actuated by forces of this kind. In applying this principle to the motion of light, we may omit the consideration of the minute curve which it describes, in its passage from a vacuum into the transparent medium, and suppose its velocity constant, when it has arrived at a sensible depth. The principle of the least action is then reduced to the passage of the light from a point without to a point within the crystal, in such a manner, that if we add the product of the right line described without into its primitive velocity, to the product of the right line described within the crystal, into its corresponding velocity, the sum may be a minimum. This principle always gives the velocity of light in a transparent medium, when the law of refraction is known, and on the other hand, gives this law, when we know the velocity. But there is a *condition*, which becomes necessary in the case of extraordinary refraction, which is, that the velocity of the ray of light in the medium must be *independent of the manner in which it has entered it*, and must be determined only by its situation with respect to the axis of the crystal, that is, by the angle which the ray forms with a line parallel to the axis." "*I have found* that the law of extraordinary refraction, laid down by Huygens, satisfies this condition, and agrees at the same time with the principle of the least action; so that *there is no reason to doubt that it is derived from the operation of attractive and repulsive forces*, of which the action is only sensible at insensible distances. The expression of the velocity to which my analysis *has conducted me* affords a *valuable datum* for determining the nature of these forces; this velocity being measured by a fraction, of which the numerator is unity, and the denominator the radius of the spheroid which is described by the light, the velocity in a vacuum being considered as unity. The velocity of the ordinary ray, in the crystal, is equal to unity divided by the principal axis of the spheroid, and is consequently greater than that of the extraordinary ray: the difference of the squares of the two velocities being proportional to the square of the sine of the angle which the latter ray makes with the axis; and this difference represents that *of the actions of the crystal* on the two kinds of rays. According to Huygens, the velocity of the extraordinary ray, in the crystal, is simply expressed by the radius of the spheroid; consequently, his hypothesis *does not agree* with the principle of the least

action; but *it is remarkable* that it agrees with the principle of Fermat, which is, that light passes, from a given point without the crystal, to a given point within it, in the least possible time; for it is easy to see that this principle coincides with that of the least action, if we invert the expression of the velocity. Thus both of these principles lead us to the law of extraordinary refraction discovered by Huygens, provided that, for Fermat's principle, we take, with Huygens, the radius of the spheroid as representing the velocity, and, for the principle of the least action, this radius be made to represent the time employed by the light in passing through a given space." "If the diameters of the spheroid are equal, the figure becomes a sphere, and the refraction resembles ordinary refraction: so that in these phenomena, nature, in proceeding from what is simple to that which is more complex, *takes the form of the ellipsis* next to that of the circle, as in the motions and the figures of the heavenly bodies."

Mr. Laplace then gives an account of the controversy between Descartes and Fermat respecting the velocity of light, and concludes his abstract with the following remarks:—

"Maupertuis, convinced by the arguments of Newton, of the truth of the suppositions of Descartes, found that the function which is a minimum in the motion of light, is not, as Fermat supposed, the sum of the quotients, but that of the products of the spaces described, by the corresponding velocities. This result, extended to the fluent of the product of the fluxion of the space into the velocity, where the motion is variable, suggested to Euler the principle of the least action, which Mr. de Lagrange afterwards deduced from the primitive laws of motion. The use which I have now made of this principle, first in order to *discover* whether or no the law of extraordinary refraction laid down by Huygens *depends on attractive or repulsive forces*, and thus to raise it into the rank of those laws which are mathematically accurate; and, secondly, to deduce mutually *from each other* the laws of refraction and of the velocity of light in transparent mediums, appears to me to be worthy *the attention* both of natural philosophers and of mathematicians."

Such is Mr. Laplace's own account of the investigations, into which he has been led by Mr. Malus's experiments; and we shall give him full credit for having demonstrated, in the original memoir, everything which he has here asserted.

The principle of Fermat, although it was assumed by that mathematician on hypothetical, or even imaginary grounds, is in fact a fundamental law with respect to undulatory motion,

and is explicitly the basis of every determination in the Huygenian theory. The motion of every undulation must necessarily be in a direction perpendicular to its surface; and this condition universally includes the law, that the time occupied in its propagation between two given points must be a minimum; or rather, more generally, the effects of the collateral undulations must always conspire the most completely, where the time occupied in their arrival at two neighbouring points in the direction of the undulations is equal, which is necessarily a condition of a minimum. Mr. Laplace seems to be unacquainted with this most essential principle of one of the two theories which he compares; for he says, that "it is remarkable," that the Huygenian law of extraordinary refraction agrees with the principle of Fermat; which he would scarcely have observed, if he had been aware that the law was an immediate consequence of the principle.

In the second place, the law of the least action is precisely the same with the law of Fermat, excepting the difference of the interpretation of the symbols. In the law of Fermat, the space is divided by the velocity, to find the time: in the principle of the least action, instead of dividing by the velocity, estimated in the Huygenian manner, we multiply by its reciprocal, to which we give the name of velocity, upon a different supposition; but the mathematical conditions of the two determinations are always necessarily identical; and the law of the least action must always be applicable to the motions of light, as determined by the Huygenian theory, supposing only the proportion of the velocities to be simply inverted.

Mr. Laplace has therefore given himself much trouble to prove that coincidence in a particular case, which must necessarily be true in all possible cases. In a person who seems to delight in long calculations, this waste of labour may easily be excused. A Turk laughs at an Englishman for walking up and down a room when he could sit still; but Mr. Laplace may walk about, and even dance, as much as he pleases, in the flowery regions of algebra, without exciting our smiles, provided that he does no worse than return to the spot from which he set out: but when, in the rapidity of his motion, his head

begins to turn, it is time for the spectators to think of their own safety.

Not satisfied with this important discovery, that the extraordinary refraction is consistent with the principle of the least action, he proceeds to infer, that "there is no reason to doubt" that this refraction *depends* on the immediate operation of attractive and repulsive forces. With as much reason might it be asserted, that because the sound produced by a submarine explosion is, in all probability, regularly refracted at its passage into the air, the sound must be attracted by the air, or repelled by the water.

Nor would such a conclusion be by any means *equally* unwarrantable with that which Mr. Laplace has drawn; a simple attractive or repulsive force, acting on a projected corpuscle in a direction perpendicular to the surface of the water, would be sufficient to explain such a refraction; but Mr. Laplace has not attempted to describe the kind of force which would be capable of producing the effects in question with respect to light. He contents himself with saying, that the velocity within the crystal must depend only on the situation of the ray of light with respect to the axis, and that this is a necessary "condition" of the refraction. The musician celebrated by the epigrammatist, thought it "a necessary condition" that a string and its octave should vibrate together, because the materials of both strings were taken from the same sheep; and he applauded himself on the sufficiency of his explanation with about as much justice as our author. In fact, the deduction of this "condition," from any assignable laws of attraction, is *the only difficulty* in the question; and this is the "dark passage" which the "commentators" have shunned.

But the insertion of such a condition seems even to exempt the problem from being directly amenable to the law of the least action. We apprehend that this law is only demonstrable, from mechanical principles, in cases of the operation of attractive forces directed to a certain point, whether fixed or variable, or acting in parallel lines, so that the velocity, between the same parallel or concentric surfaces, may be always the same, whatever its direction may be; it cannot therefore be applied,

without the most unjustifiable violence, to cases in which the velocity deviates most essentially from this description.

When we consider that, upon such grounds as these, a mathematician of the first celebrity professes to have elevated the principle of Huygens "to the dignity of a rigorous law," we cannot help being reminded of his Egyptian predecessor, who had "spent forty years in unwearied attention to the motions and appearances of the celestial bodies, and had drawn out his soul in endless calculations," in order to be persuaded at last, that "the sun had listened to his dictates, and had passed from tropic to tropic by his direction; that the clouds, at his call, had poured their waters, and the Nile overflowed at his command."

Mr. Laplace very justly remarks, that nature in these phenomena, as well as in those of astronomy, has *taken* the form of the ellipsis next to that of the circle. But in astronomy, we know *why* nature "has taken the form of the ellipsis," since the elliptic form depends on the simple law of the variation of the force of gravitation: in these phenomena of extraordinary refraction, on the contrary, no satisfactory attempt has been made to obtain any such simplification. A solution of this difficulty might, however, be deduced, upon the Huygenian principles, from the simplest possible supposition, that of a medium more easily compressible in one direction than in any direction perpendicular to it, as if it consisted of an infinite number of parallel plates connected by a substance somewhat less elastic. Such a structure of the elementary atoms of the crystal may be understood by comparing them to a block of wood or of mica. Mr. Chladni found that the mere obliquity of the fibres of a rod of Scotch fir reduced the velocity, with which it transmitted sound, in the proportion of 4 to 5. It is, therefore, obvious that a block of such wood must transmit every impulse in spheroidal, that is oval, undulations: and it may also be demonstrated, as we shall show at the conclusion of this article, that the spheroid will be truly elliptical, when the body consists either of plane and parallel strata, or of equidistant fibres, supposing both to be extremely thin, and to be connected by a less highly elastic substance; the spheroid being in the former case oblate

and in the latter oblong. It may also be proved, that while a complete spheroidal undulation is everywhere propagated by the motion of the particles in a direction perpendicular to its surface, a detached portion, like a beam of light or of sound, will proceed obliquely, in the rectilinear direction of a diameter.

It has often been asserted, and Mr. Laplace repeats the charge, that the phenomena, which are observable upon the transmission of light through a second portion of the crystal, are "inexplicable" upon the Huygenian theory. It is true that they have not yet been explained; but what right has Mr. Laplace to suppose, that this theory has yet attained to its utmost degree of perfection in every other respect, under all the obloquy with which it has been loaded? Had the more prevailing system afforded anything like an explanation of the perfect ellipticity of the undulations, it would have been opprobriously objected to the Huygenian system, that it was incapable of accounting for this circumstance; and the reproach would have remained hitherto unanswered. It may, however, be observed, that an undulation, which has passed through a crystal, is not, as some authors have taken for granted, alike on all sides; nor can it be proved, that the difference of its curvature, in its different sections, may not be sufficient to produce all the observable modifications of its subsequent subdivision.

These considerations, we trust, will amply justify us in giving it as our opinion, that Mr. Laplace has, in this memoir, been not a little superficial in his arguments, and extremely precipitate in his conclusions. We must again lament the serious evils which are likely to arise, and which in this case have actually arisen, from that unfortunate "rage for abstraction," which we have already noticed as too universally prevalent. "To avoid such paralogisms and such whims," said the late Professor Robison on a similar occasion, "we are convinced that it is prudent to deviate as little as possible, in our discussions, from THE GEOMETRICAL METHOD."

The proposition, which we left to be demonstrated, was this: "an impulse is propagated through every perpendicular section of a lamellar elastic substance in the form of an elliptic undulation." The want of figures will, perhaps, render the demon-

stration somewhat obscure, but the deficiency may easily be supplied by those who think it worth their while to consider the subject attentively.*

"When a particle of the elastic medium is displaced in an oblique direction, the resistance, produced by the compression, is the joint result of the forces arising from the elasticity in the direction of the laminæ, and in a transverse direction: and if the elasticities in these two directions were equal, the joint result would remain proportional to the displacement of the particle, being expressed, as well in magnitude as in direction, by the diagonal of the parallelogram, of which the sides measure the relative displacements, reduced to their proper directions, and express the forces which are proportional to them. But when the elasticity is less in one direction than in the other, the corresponding side of the parallelogram expressing the forces must be diminished, in the ratio which we shall call that of 1 to m; and the diagonal of the parallelogram will no longer coincide in direction with the line of actual displacement, so that the particle displaced will also produce a lateral pressure on the neighbouring particle of the medium, and will itself be urged by a lateral force. This force will, however, have no effect in promoting the direct propagation of the undulation, being probably employed in gradually changing the direction of the actual motions of the successive particles; and the only efficient force of elasticity will be that which acts in the direction in which the undulation is advancing, and which is expressed by the portion of the line of displacement, cut off by a perpendicular falling on it from the end of the diagonal of the parallelogram of forces; and the comparative elasticity will be measured by this portion, divided by the whole line of displacement. Calling the tangent of the angle formed by the line of displacement with the line of greatest elasticity t, the radius being 1, the force in this line being also 1, the transverse force will be expressed by $m\,t$, the line of displacement by $\sqrt{(1+t\,t)}$ its diminution by $\frac{(1-m)\,t\,t}{\sqrt{(1+t\,t)}}$, the diminished portion, which measures the force, by $\sqrt{(1+t\,t)} - \frac{(1-m)\,t\,t}{\sqrt{(1+t\,t)}}$, and the elasticity, in the given direction, by $\frac{1+m\,t\,t}{1+t\,t}$. Hence it follows, that the velocity of an impulse, moving in that direction, will be expressed by $\sqrt{\frac{1+m\,t\,t}{1+t\,t}}$.

* The principle involved in this demonstration forms a capital step in the undulatory theory. See Dr. Whewell's 'History of the Inductive Sciences,' vol. ii., p. 412.—*Note by the Editor.*

"It is next to be proved, that the velocity of an elliptical undulation, increasing so as to remain always similar, by means of an impulse propagated always in a direction perpendicular to the circumference, is such as would take place in a medium thus constituted. It is obvious that the increment of each of the diameters of the increasing figure must be proportional to the whole diameter; and this increment, reduced to a direction perpendicular to the curve, will be proportional to the perpendicular falling on the conjugate diameter, which will measure the velocity. We are therefore to find the expression for this perpendicular, when it forms an angle with the greater axis, of which the tangent is t. Let the greater semi-axis be 1, and the smaller n: then the tangent of the angle, formed with the greater axis by the conjugate diameter, being $\frac{1}{t}$; the tangent of the angle subtended by the corresponding ordinate of the circumscribing circle is found $\frac{1}{nt}$, and the semi-diameter itself, equal to unity, reduced in the ratio of the secants of these angles, that is, to $n\sqrt{\frac{1+tt}{1+nntt}}$; but, by the known property of the ellipsis, the perpendicular required is equal to the product of the semi-axes divided by this semi-diameter, that is, to $\sqrt{\frac{1+nntt}{1+tt}}$: we have, therefore, only to make $n\ n = m$, and the velocity in the given medium will always be such as is required for the propagation of an undulation, preserving the form of similar and concentric spheroids, of which the given ellipsis represents any principal section.

"If the whole of the undulation were of equal force, this reasoning would be sufficient for determining its motion; but when one part of it is stronger than another, the superiority of pressure and motion will obviously be propagated in the direction of the actual resistance produced by the displacement of the particles, since it is this resistance which carries on the pressure, and consequently propagates the motion. It is very remarkable, that the direction of the resistance will be found, on the supposition which we have advanced respecting the constitution of the medium, to coincide everywhere with the diameters of the ellipsis, when the displacement is perpendicular to the surface. For it is proved by authors on conic sections, that the subnormal of the ellipsis is to the absciss, as the square of the lesser axis is to the square of the greater, that is, in this case, as $n\ n$ to 1, or as m to 1; but if we divide the ordinate in the same ratio of m to 1, and join the point of division with the extremity of the subnormal, this line, which

will evidently be parallel to the diameter, will express, as we have already seen, the direction of the force, when the normal represents that of the displacement. An immediate displacement in the direction of any diameter, making an angle with the axis of which the tangent is t, would give a velocity of $\sqrt{\frac{1+mtt}{1+tt}}$, while the increment of the diameter would require a velocity of $\sqrt{\frac{m+mtt}{m+tt}}$, which does not vary in the same proportion. It must, however, be remembered, that the rectilinear direction of the beam is not supposed to depend on this circumstance alone: Huygens considers each point of the surface of the crystal, on which a beam of light impinges, as the centre of a new undulation, which spreads, in some measure, in every direction, but produces no perceptible effect, except where it is supported by, and co-operates with, the neighbouring undulations; that is, in the surface which is a common tangent of the collateral undulations; but if this principle were applied without the assistance of the obliquity of force, which we have deduced from the supposition of a stratified medium, it would lead us to expect that the elementary impulses, being propagated in a curvilinear trajectory, might be intercepted by an object not situated in the rectilinear path of the beam; a conclusion which is not warranted by experiment."

It is not probable that any other supposition respecting the constitution of the medium, in the Huygenian theory, could afford a result so strikingly coincident with the phenomena of extraordinary refraction; and the most decided advocates of the projectile system must allow, that there is scarcely a chance, especially after Mr. Laplace's fruitless researches, of its being capable of an application by any means comparable to this for precision and simplicity. But it must be remembered, that we have been considering a single class of phenomena only; the two rival theories must be viewed in a multiplicity of various lights, before a fair estimate can be candidly formed of their comparative merits; and we are not arguing for a decision in favour of either, but for a temperate suspension of judgment, until more complete and more satisfactory evidence can be obtained.*

* We must do Mr. Laplace the justice to observe, that since this article was written, he has published, in the Memoirs of Arcueil, another paper on this subject, in which the name of Dr. Wollaston is mentioned with due respect. The same volume contains also an account of some highly interesting and important experiments

of Mr. Malus, on the apparent polarity of light, as exhibited by oblique reflexion, which present greater difficulties to the advocates of the undulatory theory than any other facts with which we are acquainted.*—*Note by Dr. Young.*

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* It would appear that Dr. Young applied to Dr. Wollaston for his permission to introduce his name in this criticism. The following is his reply:—

"My dear Sir,

"I cannot, without a most egregious share of *mauvaise honte*, object to your asserting my claims to originality in the verification of the Huygenian law: but I certainly must object to your burying your own claims to an original investigation in a shroud of anonymous criticism.

"For though Laplace may be answered with most *effect in this country* by such a critique, the *filius neminis* which follows in its suite may be long before it receives the respect due to your legitimate offspring.

"Surely this ought to be published first, not by X. Y., but by T. Y., *pro bono publico;* and then Laplace might be attacked with at least as good effect with the same weapons. It is possible that Malus may have improved upon my experiments, and may have given Laplace results that have greater pretensions to accuracy than mine: but he is probably not so correct in the angle of the crystal. If he declines adopting my measures, and if Malus suppresses the real origin of his experiments, Laplace's silence is more excusable.

"Yours ever,

Note by the Editor. "W. H. WOLLASTON."

No. XIII.

REVIEW OF THE

"MÉMOIRES DE PHYSIQUE ET DE CHIMIE DE LA SOCIÉTÉ D'ARCUEIL."

VOLS. I. AND II.

From the Quarterly Review for May, 1810, vol. iii., p. 462.

THESE volumes are composed exclusively of the productions of a select decad of the most celebrated men of science resident at Paris, who meet once a fortnight to pass the day together, in making and discussing philosophical experiments, at the house of the elder Berthollet, now a count of the French empire, situated at Arcueil, in the neighbourhood of a villa which has lately been purchased by Count Laplace. These two gentlemen may be considered as the fathers of the society: the other members are Biot, Gay Lussac, Humboldt, Thénard, Decandolle, Collet Descotils, A. B. Berthollet, and Malus.

The formation of private associations of this kind seems to be a natural step in the division of literary labour. In this country we have had abundant instances both of independent and of affiliated societies, for the cultivation of particular departments of science, all of which had remained for many years united, as objects of the attention of the Royal Society alone: and several of these associations have already been productive of no contemptible contributions to the advancement of the several sciences to which they have been respectively devoted.

The researches of the Society of Arcueil extend to the most important and interesting of the topics which constitute the occupation of the first class of the Institute of France: the individuals who compose it being the most eminent members of the Institute in their different departments, they must naturally have the same facts and opinions to produce and to compare in

both capacities. Indeed a great part of the essays, which are presented to the public in abstract in the Mémoires d'Arcueil, has been read in a more extended form to the National Institute; nor is it likely that any jealousy will be excited in this celebrated body from the competition: it has always shown a laudable liberality with respect to the publication of the papers laid before it; rightly judging that the paltry consideration of copyright, and the reservation of the earliest notification of its discoveries, is wholly unworthy the care of a body devoted to the cultivation and at the same time to the general dissemination of science.

There is not uncommonly a degree of zeal and emulation attending the pursuits of a private association, which cannot always be obtained in an equal degree by any public encouragement held out to science. Thus the stipends of the academicians of the Institute, which are sufficient to induce men of small fortunes and moderate wishes to devote their attention to science, are by no means calculated to call the most brilliant powers into the strongest action; and a society so constituted is more likely to do a great deal tolerably, than a little admirably. In this country we cannot boast of any very high encouragements directly held out to genius; but there is always a prospect, often indeed delusive, that talents may raise their possessor to situations of eminence and dignity, in whatever profession they may be exhibited; and the remote chance of a high prize seems to be more likely to produce extraordinary exertions, than a greater certainty of an inferior one. The advantages which are derived, in some of our colleges, from a moderate degree of success in mathematical and classical pursuits, are somewhat analogous, in their effects, to the encouragements which have been granted to scientific bodies on the continent, by their respective governments: but, including all the remote prospects of promotion, the prizes may on the whole be considered as much higher; they are, however, in general adjudged at so early an age, that their influence as a stimulus to application is but of short duration.

We do not intend, by this remark, to imply a censure of the system adopted by our universities in the adjudication of their

honours and rewards; for it must be remembered, that the *advancement* of learning is by no means the principal object of an academical institution: the *diffusion* of a respectable share of instruction in literature and in the sciences, among those classes which hold the highest situations, and have the most extensive influence in the state, is an object of more importance to the public than the discovery of new truths, or the invention of new modes of illustrating those which are already established; and this object appears to require, for its attainment, a continued succession of instructors, possessing precisely those qualifications which are most immediately encouraged by the present system. We might, perhaps, even venture to assert, that in almost all departments of learning, the *elementary* doctrines are of far more practical utility than the more abstruse investigations; and that with respect to the general improvement of the talents, an intense application to a particular branch of study is as often prejudicial as advantageous. We think that we have observed numerous instances, both in public life, and in the pursuit of natural knowledge, in which great scholars and great mathematicians have reasoned less soundly, although more ingeniously, and written less elegantly, although more elaborately, than others, who, being somewhat more completely in the possession of common sense, at the same time that they had not neglected those pursuits, which are very properly considered as essential to the education of a gentleman, were still far inferior to them in the refinements of learning or of science.

The two volumes of the Memoirs of the Society of Arcueil are particularly interesting, as they contain, besides some original articles of high importance, a summary view of the principal investigations which have, during the last two or three years, employed the most celebrated of the philosophers of France. Our attention has also been more irresistibly directed to them by the manner in which they have been noticed in a well-known periodical publication, which has acquired no inconsiderable reputation in this country, even with regard to matters of science.* We are not very ambitious of obtaining the approbation of those readers, who can have attentively con-

* Edinburgh Review for February and May, 1810, vol. xv., pp. 142 and 418.

sidered the articles to which we alluded, without discovering some of their numerous errors; yet we have known instances in which the minds of some well-disposed and candid persons have been led astray, by the specious and ostentatious performances of the same school: and we think the present a favourable opportunity for examining into the validity of its pretensions to the dictatorial character which it has assumed. The humiliating confessions of our national inferiority as mathematicians, which these too liberal critics have lately held forth to the world, have not escaped the vigilance of our hereditary rivals on the continent; a translation of their reflections upon this subject has been distinguished, in an unusual manner, with a place in the Journal de Physique. In the present instance, we must do them the justice to say, that they have not been deficient in their contributions towards the support and illustration of their own propositions respecting the actual state of the sciences in Great Britain. Nor have they been altogether deficient in affording occasional opportunities of triumph to the philologists, as well as to the mathematicians of the continent: we shall not enlarge at present on this subject; but we may perhaps have occasion to meet them at a future time on the "Phœnician plains," which they have very lately introduced to our acquaintance, in defiance of Laporte du Theil and Coray, as well as in opposition to all lexicographers and grammarians.*

The contents of the first volume of the Memoirs of Arcueil, which has been published about three years, have already become generally known through various channels. The *magnetical* observations of Biot and Humboldt, which stand first on the list, are so far important as they relate to the intensity of the magnetic forces acting on the compass, which these philosophers have found to be 137 at Berlin, and 125 at Rome, calling it 100 at the magnetic equator: but the position, which they have assigned to this imaginary line, seems to be less accurate than that which it occupies in Mr. Churchman's chart. Mr. Thénard's various papers on the *bile*, and on *ethers* of different

* This refers to a singular mistranslation of a passage from a fragment of Æschylus, sanctioned by the Reviewer of the *Traduction de Strabon* by Coray and Laporte du Theil: 'Edinburgh Review' for April, 1810, p. 61.—*Note by the Editor.*

kinds, contain a number of remarkable results relating to the chemical constitution of these substances. Mr. Berthollet has particularly examined the combination of sulfur with the muriatic acid, discovered by Dr. Thomson, and thinks that it ought to be considered simply as an *oxysulfureted muriatic acid.* Mr. Gay Lussac describes some interesting experiments on the *expansion of gases.* When two equal balloons were employed, and one of them being exhausted, a communication was opened with the other, the heat observed in the first was always nearly equal to the cold produced in the second, and both were nearly proportional to the density of the air concerned: but the proportion by no means held good for gases of different kinds; hydrogen, for instance, exhibiting a greater change of temperature than common air. The same gentleman has also made some observations on *evaporation,* and on the decomposition of the *sulfates* by heat. Mr. Biot finds that the air in the *bladders of fishes* is the purer in proportion as they occupy deeper parts of the ocean, consisting, in fishes which are found at great depths, of much more than half its bulk of oxygen. Mr. Berthollet describes a useful *manometer,* or rather gazometer. Mr. A. B. Berthollet shows that the *liquor of Lampadius* is, as that chemist supposed, a hydruret of sulfur, and not a carburet. Mr. Berthollet gives an account of a *cheesy substance,* obtained from muscular flesh. A short note by Gay Lussac, on the capacity of different bodies, with respect to *chemical saturation,* closes the first volume.

The second volume appears far to exceed the first in the importance of its contents. Besides those articles which we shall more particularly examine, it contains a continuation of Mr. Thénard's researches respecting the *action of acids on alcohol,* and an extension of the results of the investigation to the *neutral compounds* formed by acids with other vegetable and with some animal substances. In Mr. Berthollet's observations on the *proportions of the elements* of some combinations, allowance is particularly made for the quantity of water which has often adhered to some of these elements, when they have been supposed to be pure. This celebrated chemist has also directed his attention anew to the gases composed principally of *hydrogen*

and carbon, and agrees with some of our countrymen in the opinion, that they all contain oxygen: he is also persuaded that their composition is not limited to any fixed proportions. Mr. Decandolle gives a very simple explanation of the well-known *tendency of plants* to approach the light; observing that the calorific effects of light, by which also carbonic acid is decomposed, are accompanied by a contraction of the fibres on the side most affected, which naturally bends the young shoots; and that those plants which are not coloured by light, for instance the cuscuta, have no disposition to approach it. Mr. Gay Lussac has presented us with three memoirs, on the relation between the *oxidation of metals* and their saturation with acids, on the mutual *combination of gases*, and on the employment of nitrous gas or *nitric oxid* in eudiometry, for the foundation of all of which he seems to be wholly indebted to the ingenious theories of Mr. Dalton: he has also described a eudiometer in which an excess of nitric acid is added, with as little agitation as possible, to the mixture to be examined; and one fourth of the diminution, produced by the formation of nitrous acid, represents very accurately the quantity of oxygen contained in it. MM. Thénard and Biot have very carefully analysed the *arragonite*, which they find to be perfectly identical, in its chemical constitution, with the common rhomboidal subcarbonate of lime, although its refractive powers are considerably greater, but not in the proportion that might be expected from the still greater excess of its specific gravity. Mr. A. B. Berthollet has entered into some elaborate researches on the composition of *ammonia*, which, although not perfectly conclusive, yet appear on the whole to be unfavourable to Mr. Davy's opinion, that this substance contains an appreciable portion of oxygen. MM. Provençal and Humboldt have made a great number of very accurate experiments on the *respiration of fishes*, showing that a supply of oxygen is absolutely necessary to their existence, although a very small quantity is sufficient; that the bulk of the carbonic acid produced is considerably less than that of the oxygen absorbed, and that there is some deficiency of nitrogen. Mr. Descotils makes some practical remarks on the operation of procuring *lead from its sulfuret*: he finds that

there is considerable loss wherever any gaseous substance is present, and recommends that it be smelted by fusion with iron only, or with some of the most metallic of its ores, where it is possible. Mr. Berthollet has related, in some short notes, the results of several very interesting experiments of a miscellaneous nature. The first is on the *heat produced by percussion,* which he considers as proportional only to the permanent condensation of the substance compressed: in some cases he found that no heat was produced, and observing that the apparatus was half a degree colder than the surrounding objects, he concludes that the agitation of percussion must increase the conducting power for heat; but this can scarcely be deemed a justifiable inference, since the elevation of temperature generally observed was 10 or 12 degrees, in comparison with which the difference of half a degree must have been wholly inconsiderable, especially as the time of contact was extremely short. The oriental *bezoar* he finds to be a concretion, probably of woody fibres. His experiments on the *respiration of small animals* agree with those of MM. Allen and Pepys, in exhibiting an evolution of nitrogen. In order to examine the truth of Mr. Dalton's hypothesis concerning the *constitution of mixed gases,* he left several combinations for some days in bottles, which communicated by a narrow tube, and in some cases there remained to the last very well marked differences in their respective contents: but he found that hydrogen mixed more readily with every other gas than any third species would do. He has confirmed the general result of the doctrines of Dalton, Wollaston, and Thomson, respecting the *proportion of combinations,* but thinks that it admits of many exceptions and modifications: and lastly, he has ascertained, that a portion of *nitrogen* adheres so strongly to charcoal, as always to form a part of the gas which is expelled from it by a strong heat in coated glass vessels; so that we can by no means consider charcoal in its common state, as at all approaching to a simple elementary substance. Such are the outlines of the results of the principal investigations related in this volume, besides those which we are now to proceed to notice somewhat more in detail.

Experiments on the Propagation of Sound in Vapours.

By Mr. Biot.

When a liquid of any kind is introduced into the vacuum of a barometer, the mercury is more or less depressed, according to the nature of the liquid, and to the temperature of the atmosphere, the elasticity of the vapour, which rises from the liquid, assisting the weight of the mercury in counteracting the atmospherical pressure: and if we cause the space occupied by the vapour to be diminished or increased, by adding to or taking from the quantity of mercury in the bason of the barometer, or by altering the inclination of the tube to the horizon, the effective height of the mercury will remain in all cases the same, provided that there be an excess of the liquid in the tube. Under these circumstances, therefore, the elasticity of the vapour is not increased by compression, nor diminished by rarefaction; a deposition of a part of the vapour taking place in the one case, and an additional evaporation in the other. Hence Mr. Biot argues, that a vapour simply so constituted could not transmit sound, since its elasticity would not be increased at the part which receives the positive impulse of the vibrating body, nor diminished where the body is retreating: and the only way in which he thinks that sound could be conveyed by such a medium, is by means of the heat evolved by its compression, which must enable it to retain the elastic form with a temporary increase of density, where the positive impulse is to be transmitted. Hence, finding that vapour does actually transmit sound very perceptibly, that of ether indeed almost as well as atmospherical air, he infers that such an elevation of temperature must be produced by the compression of vapour in general, and he concludes also that a similar effect must take place in the ordinary transmission of sound through the atmosphere, according to the explanation which Mr. Laplace has given of the difference between the observed velocity of sound, and the velocity calculated from the simple elasticity of the air, as exhibited by slow compression.

We do not wish to withhold our approbation of Mr. Biot's diligence in attempting to reduce the ingenious theory of Laplace

to the test of experiments; but we must confess that, in the present instance, the experiments, however interesting in themselves, appear to be both inconclusive and superfluous, as applied to the theory in question.

We think them inconclusive, because it seems manifest to us that sound might be transmitted by a vapour, without the property of the evolution of heat by simple compression. The first stroke of the vibrating body would cause a slight deposition of the liquid, and the portion thus deposited would by no means be instantaneously converted into vapour, upon the retreat of the body. The space would therefore be left a little undersaturated, and the sound would be transmitted without further impediment: for a vapour, below the point of saturation, possesses all the properties of a permanent gas. Besides, the conversion of a part of the vapour into a liquid would unavoidably be attended by the extrication of a certain portion of heat, which would increase the elasticity of the remaining gas, without any immediate evolution of heat by its compression. It may also be shown, that even in the actual circumstances of a vapour capable, in all probability, of being heated by compression and cooled by expansion, the space must inevitably be somewhat undersaturated during the transmission of every sound through it. Whenever a gas, nearly saturated with humidity, is expanded, there is a deposition of visible moisture; and we have every reason to believe, according to the experiments of Mr. Dalton, that the same must happen to a vapour unmixed with a more permanent gas: consequently the expansion of the vapour, where it has followed the receding particles of the vibrating body, must necessarily be attended by a deposition of a minute portion of the liquid, which will not instantly evaporate; so that the vapour will never remain precisely at the utmost point of elasticity which the general temperature is capable of supporting, and will therefore never be, mathematically speaking, in the circumstances which Mr. Biot supposes.

But even if it be granted that these experiments have a tendency to support Mr. Laplace's theory, we cannot help thinking that their support is perfectly unnecessary. The velocity of sound must obviously depend on the temporary elasticity of the

medium at the respective points concerned, which is only required to continue for a time almost inconceivably small, much smaller than that which would be sufficient to allow the diffusion of the heat and cold produced by compression and expansion. We have no instruments delicate enough to measure very precisely the magnitude of the changes of temperature produced in such cases; but, in the first place, it is easily proved from the well-known circumstance of the constant appearance of vapour in the receiver of the air-pump, that at least 20 or 30 degrees of cold must be produced by the expansion of a portion of air to twice its bulk, since a depression of temperature equal to this is required, as appears from Mr. Dalton's tables, in order to produce such a deposition of moisture in a portion of air thus expanded, even when it has been previously in the utmost possible state of humidity: and secondly, Mr. Dalton has very ingeniously inferred, from the rapidity with which a thermometer begins to sink in the first instance, that about 50° are actually exhibited; and Dr. Young has shown that the results of some of Mr. Dalton's experiments make it probable that the effect is still greater than he has supposed; so that it may be considered as strictly demonstrable, that the velocity of sound must be increased about one-seventh from this cause, while the observed increase is about one-fifth. Since therefore it may be proved, that at least $\frac{1}{2}$ of the increase must arise from the cause so happily suggested by Mr. Laplace, it appears to be more natural to suppose, that the whole difference arises from the same cause, the operation of which cannot be so accurately traced under any other circumstances, than to imagine that any second mystery still remains, to be unveiled by future conjectures.

Since the velocity of sound is not in the least affected by any alteration in the density of the air, it follows that equal degrees of compression must produce equal elevations of temperature in all cases; and that in Gay Lussac's experiments on portions of air of different densities, the apparent differences must have arisen principally from the imperfection of the indications of the thermometers.

We cannot avoid noticing here the utter darkness that seems

to have enveloped the secret tribunal, which lately passed its sentence of condemnation on the theory of its own idol, Laplace. It is asserted in support of this sentence, that in order to have the elasticity of the aërial wave augmented in the proportion of two to three, it would be necessary that the temperature should be raised "125° of Fahrenheit's scale;" and that for this purpose the successive portions of air must be compressed into "one-fifth of their usual space," by means of a velocity of "impact equal to 3350 feet in a second." We maintain, that instead of 125°, Mr. Laplace's theory does not require, in any common case, an elevation of one single degree, or even of *half* a degree of Fahrenheit. It is not "the elasticity" which is to be augmented one half, but *its excess* above the mean pressure; and this excess, or the actual condensation, is probably seldom so great as one thousandth of the whole density; and it will be sufficient if such a condensation be accompanied by an elevation of *one-fourth* of a degree, in order to justify the opinion of this celebrated philosopher.

We could easily pardon a mistake of this kind in a hasty opinion expressed privately by an individual; although from an author of any description, however inexperienced and unassuming, we should expect a greater degree of attention: but when such errors are dictatorially proclaimed by an arbiter of science, as the ultimate decisions of critical accuracy, and in defiance of the authority of a mathematician, who, as we are taught to believe, at the distance of a few pages only, has so "few rivals," that "Lagrange is the only man now living who may be fairly placed by his side," we cannot help feeling the truth of the observation, "that the foolhardy proceed *boldly, because blindly.*"*

An attempt, equally futile, has been made by the same critic, where he endeavours to improve on the refined calculations of Mr. Lagrange respecting the velocity of sound. The chain of reasoning, by which these calculations are established, is unimpeachable in the circumstances to which it is applied: the observations, which the critic has made, on the initial motion of

* Edinburgh Review, vol. xv., p. 432. The Reviewer was said to be Professor Leslie.—*Note by the Editor.*

the separate particles of the medium, are verbally true, but effectively fallacious, since the contemporaneous motion, to which they relate, although it might take place in the last of a system of a very small number of atoms, "A, B, C, D, and E," yet would become absolutely imperceptible, if their number were only increased to as many particles of air as would stand on the point of a needle.

"Professor Leslie" is certainly much obliged to his *kind countryman* who has endeavoured, in his account of this paper, to support the tottering hypothesis of aërial undulations, as contributing to the transfer of radiant heat. These undulations are supposed to be "gentle," and not to excite in the air "the tremor which causes noise:" yet they are imagined to be violent enough to transfer so much heat, as will elevate the temperature of a body several hundred degrees, by the simple effect of the progressive condensation, as producing a change of capacity in the air, which gives out this heat to the air in contact with it, prepared for its reception by a favourable and apparently *spontaneous* dilatation, while the condensation seems only to be produced by the pressure of the heat, first thrusting the air before it, and then penetrating it without resistance. If anything is necessary for the confutation of so unintelligible and so unprofitable a speculation, after the full establishment of Dr. Herschel's discovery of invisible solar heat, and after some late observations on the actual transmission of some portion of the heat of a fire by radiation through lenses, it may be found in Mr. Davy's elegant experiment on the radiation of the heat excited by galvanism, in the vacuum of an air-pump, where the effect of reflection is not only not inferior to that which takes place in the open air, but incomparably greater and more rapid.

It is indeed remarkable that so much ingenuity and happy invention, as are exhibited in Mr. Leslie's work on heat, should be alloyed by so much inaccuracy of reasoning, and so much want of mathematical precision. Among many instances of this kind, we will only adduce one passage, p. 127. "If," says this author, "an ivory ball strikes against another of equal weight, there should, according to the common theory, be an exact transfer of motion. But if the velocity of the impinging ball

be very considerable, so far from stopping suddenly, it will recoil back again with the same force, while the ball which is struck will remain at rest." In other words the common centre of inertia, which was moving forwards before the collision, will be made to move backwards after it. Now we have been taught by the laws of motion, laid down in the Principia, that "Quantitas motus, quæ colligitur capiendo summam motuum factorum ad eandem partem, et differentiam factorum ad contrarias, non mutatur ab actione corporum inter se:" and we must unavoidably deny the truth either of this fundamental law of motion, or of the observation recorded by Professor Leslie. In fact it is perfectly obvious that the experiment has never been made, and never can be made, with either of the balls absolutely at rest.

On the Motion of Light in Transparent Mediums. By Mr. Laplace.

We should have had very little to say of this essay, in addition to the remarks inserted in our 4th number, p. 337,* on the abstract of it before published in the Journal de Physique; since the farther details of calculation, which it contains, present no difficulties, and consequently display no ingenuity: but here again our attention has been particularly excited by some supposed improvements on the theory of extraordinary refraction, which have been suggested in this country, and we cannot refrain from inquiring how far these improvements are real.

Entertaining the opinion which we have already ventured to express on the subject, we cannot hesitate to agree in the sentence, that the "present memoir is grounded on assumptions," at least "as gratuitous and arbitrary, as those involved in the hypothesis with which it is contrasted." But we were not a little surprised in reading that the phenomena in question might "admit of a very simple investigation, from the fundamental principle of accelerating or retarding forces;" and we were utterly confounded, at first sight, with the next sixteen lines of the paragraph,† in which, as we are told, the law of extraordinary

* Supra, p. 220. † In the Review quoted above, p. 426.

refraction is at once deduced from that principle, "without requiring any more aid of the integral calculus." It is characteristic of a great master to obtain the most striking results by the most simple means: in the present instance, the result is far more satisfactory than that of the original memoir; and we were flattering ourselves for a moment with the idea, that at least one of our countrymen, who had thus happily succeeded where Laplace had failed, would deserve to be placed between him and his great "rival," in that seat which the "Emperor of half of Europe" was once delighted to occupy. But our exultation was of short duration: we soon perceived that the mode of reasoning employed would serve equally well for any other imaginable purpose, and that the apparent brevity of the statement could not be considered as surprising, since a demonstration which proves nothing may easily be concise. It is advanced as one step of this argument, that the *extent* of the action of the extraordinary force, exhibited by the Iceland crystal, "*is reduced*" in the ratio of the cosine of the inclination of the ray. But *why* is the space of action thus reduced? Only because it is necessary for the success of the demonstration that it should be so; for *how* it should become reduced in this ratio, rather than in any other, the critic does not inform us, nor have we any means of discovering; and it appears to be as unwarrantable to assume such a reduction, as it would be to take for granted the original proposition as self-evident. It is merely the desire of pointing out one more of a multitude of errors that has led us to make this objection; for the question implicates a material point in the comparison of the two theories of light, which the next paper will require us to institute.

On a Property of reflected Light; and *On a Property of the repulsive Forces which act on Light.* By MR. MALUS.

The discovery related in these papers, appears to us to be by far the most important and interesting that has been made in France, concerning the properties of light, at least since the time of Huygens; and it is so much the more deserving of notice, as it greatly influences the general balance of evidence,

in the comparison of the undulatory and the projectile theories of the nature of light.*

It was known to Huygens and to Newton, that a ray of light, transmitted and divided by one piece of Iceland crystal, or rhomboidal subcarbonate of lime, was either subdivided, or not, by a second piece, according to the relative position of the two crystals; so that if we looked down through both of them, and the obtuse angle of one was situated on the north side of the ray, and that of the other on the north-east side, four images of any object would be seen; and only two if the obtuse angle of the second, was either on the north side or on the east. Now in the simple Huygenian theory of an undulation resembling that of sound, the ray must be alike on every side, as well after as before its passage through the first crystal; nor can it be imagined how its affections can be different with respect to north and north-east, or to any other points of the compass; and this was advanced by Newton as an objection, which Huygens had not been able to overcome. We ventured to suggest, on a former occasion,† that the curvature of the undulation, or in other words, the divergence of the light, might possibly be different in different directions: now Mr. Malus's experiments are precisely such as to afford an answer to this suggestion; since they show that the divergence is absolutely unconcerned in the phenomena, and that a similar division of the light may be produced by simple reflection from a plane surface where no change of divergence takes place in any direction.

This statement appears to us to be conclusive with respect to the insufficiency of the undulatory theory, in its present state,

* In a letter addressed by Malus to Dr. Young as Foreign Secretary of the Royal Society, returning thanks for the award of the Rumford medal for his discovery of the Polarization of Light, in which he gives an account of some further observations, he adds, at the conclusion of them: "Je ne regarde pas la connaissance de ces phénomènes comme plus favorable au système de l'émission qu'à celui des ondulations. Ils démontrent également l'insuffisance des deux hypothèses; en effet, comment expliquer dans l'une ou dans l'autre pourquoi un rayon polarisé peut traverser sous une certaine inclinaison un corps diaphane, en se dérobant totalement à la réflexion partielle qui a lieu à la surface de ces corps dans les cas ordinaires?" "It was after the difficulties, which the discovery of polarization created, had come into view," says Dr. Whewell, "and before their solution had been discovered, that we may place the darkest time of the history of the undulatory theory." It was reserved for Dr. Young, at a later period, to suggest the hypothesis, by which those difficulties have been overcome.—*Note by the Editor.*

† Supra, p. 229.

for explaining all the phenomena of light. But we are not therefore by any means persuaded of the perfect sufficiency of the projectile system: and all the satisfaction that we have derived from an attentive consideration of the accumulated evidence, which has been brought forward, within the last ten years, on both sides of the question, is that of being convinced that much more evidence is still wanting before it can be positively decided. In the progress of scientific investigation, we must frequently travel by rugged paths, and through valleys as well as over mountains. Doubt must necessarily succeed often to apparent certainty, and must again give place to a certainty of a higher order; such is the imperfection of our faculties, that the descent from conviction to hesitation is not uncommonly as salutary, as the more agreeable elevation from uncertainty to demonstration. An example of such alternations may easily be adduced from the history of chemistry. How universally had phlogiston once expelled the aërial acid of Hooke and Mayow! How much more completely had phlogiston given way to oxygen! And how much have some of our best chemists been lately inclined to restore the same phlogiston to its lost honours! although now again they are beginning to apprehend that they have already done too much in its favour. In the mean time, the true science of chemistry, as the most positive dogmatist will not hesitate to allow, has been very rapidly advancing towards ultimate perfection.

The outline of Mr. Malus's important discovery may be thus familiarly represented. Suppose the altitude of the sun on the meridian to be 19° 10′, and a plate of glass, not silvered, to be so placed, as to reflect a ray of his light directly downwards: then if a second plate be fixed below and parallel to it, this plate will again reflect the descending ray into a direction parallel to the original one, and nothing remarkable will happen. But if we turn round this second plate, without altering its inclination to the horizon, as soon as it faces the east or the west, it will no longer reflect any part of the light, either from its anterior or from its posterior surface: when, however, it has made half a revolution, and fronts the south, it will again reflect the usual proportion of the incident light; and in the inter-

mediate positions, the reflection will be more or less perfect, as the reflected ray approaches more or less to the plane of the meridian. If now, instead of the second plate, we place a piece of Iceland crystal with its principal section in the plane of the meridian, the whole of the reflected ray will be transmitted by the ordinary refraction; but if we turn round the crystal till the direction of its principal section become east and west, the ray will now be subject to the extraordinary refraction only; and in all intermediate situations of the crystal, it will be divided into two portions. Mr. Malus has entered into several more particular details, respecting the results of similar experiments under various circumstances; but they do not add materially to the interest of the facts as thus simply stated.

The angle of incidence at which this modification takes place the most completely, is different for substances of different densities: for water it is 52° 45′; for glass 54° 35′, and for Iceland crystal 56° 30′. Black substances, such as polished ebony, have a similar property; but metals are entirely destitute of it. When a modified ray is reflected by a metallic mirror, so as to continue in its principal section, or to proceed in a plane perpendicular to it, it still retains its properties: but if its new direction be equally inclined to both these planes, its modification will be destroyed.

Mr. Malus has discovered, that in all doubling crystals, one of the refractions is always of the extraordinary kind, and that whether we employ carbonate of lead, sulfate of barita, crystallized sulfur, or rock crystal, the modifications which take place are precisely of the same nature. He has also ascertained that the internal reflection of the doubling crystals causes, in general, a further subdivision of the light reflected.

We are perfectly satisfied, from our own observation, of the accuracy of Mr. Malus's account of his principal experiment; but we are by no means disposed to agree with him in believing that the modified light is wholly transmitted by the surface, where it is in no degree reflected: on the contrary, we are inclined to think, that the portion usually reflected is in this case wholly absorbed, if not destroyed. We will not presume to oppose our authority to that of Mr. Malus; but he has been so

little particular in the detail of his experiments, that we are at liberty to doubt of the validity of some of his conclusions. By employing six or eight successive transmissions through as many parallel plates, the question might be easily decided; and as far as we have examined the phenomena, our results have differed in this respect from Mr. Malus's statements, which he appears rather to have set down as the natural consequences of other facts, than from direct experiment.

Mr. Malus observes very truly, that the ordinary phenomena of optics may be explained, either according to the hypothesis of Huygens, or by the doctrine of Newton: but he thinks that those properties, which he has discovered or confirmed, are only capable of an explanation from a polarity, such as was attributed by Newton to the particles of light; and for this purpose he lays down a law respecting the position of their supposed axes, which he appears to consider as satisfactory, but which we cannot help thinking manifestly and utterly inadequate to the solution of any of the difficulties.

It seems to be undeniable that the general tenor of these phenomena is such, as obviously to point at some property resembling polarity, which appears to be much more easily reconcileable with the Newtonian ideas than with those of Huygens. We must, however, observe, not only that the admission of the projectile theory is by no means sufficient for the explanation of Mr. Malus's experiments; but also, on the other hand, that there is another very extensive class of phenomena, which seems to lead us even more directly to the doctrines of the Huygenian school, than those which Mr. Malus has discovered, appear to divert us from them. We allude to the multiplicity of facts, which are referable to the general law of the mutual destruction of two portions of light, some slight rudiments of which are to be found in the works of Grimaldi, and which has been particularly investigated and extended by our countryman, Dr. Young. It has been justly conceded, that "we should not hastily reject even the wildest hypothesis;" for, "if a hypothesis be not allowed to warp the understanding, it may serve at least usefully to connect certain insulated facts." The truth of this observation is shown in a remarkable manner

by the assistance which Dr. Young has derived from the Huygenian theory, in the discovery and establishment of a law which reduces to a single principle, and explains with a degree of accuracy, in general perfectly mathematical, and always within the probable limits of the errors of observation, the phenomena, before insulated, of the colours of thin plates, of thick plates, of mixed plates, and of inflected or diffracted light, in an infinite variety of forms. This law is not only the necessary consequence of a doctrine like that which has been founded on the theory of Huygens, but is also accompanied by some other conditions immediately connected with that theory; and it is rendered still more inseparable from it, by its extension to the chemical phenomena of the invisible blackening rays, which could not be explained, upon the Newtonian doctrine of the undulatory nature of the sensation only of light, as transmitted by the optic nerve.

Of the phenomena of light which are more commonly observed, the greater part will admit an explanation equally satisfactory from either of the theories; others, although perhaps not absolutely incompatible with either, appear to us to be more naturally referable to the Huygenian than to the Newtonian. The effects of simple reflection and refraction belong to the former of these divisions: those of the dispersion of the rays of different colours may also be compared, either with the different velocities, acquired by projectiles of different magnitudes, in a resisting medium, or with those of waves of different breadths, spreading on the surface of an imperfectly elastic liquid. The transmission of light, with little interruption, through the densest transparent substances, affords a difficulty of the same kind in the Newtonian theory, as the aberration of the stars in the Huygenian: in the first instance, the ultimate atoms of matter must either be supposed permeable to light, or to be scattered at distances inconceivably great, in comparison to their own magnitudes, through the apparent dimensions of the solid bodies: in the second, the porosity of the substances concerned needs not by any means to be supposed so excessive; but there is some difficulty in conceiving the free and rapid passage of the ethereal medium through the densest

bodies, at the same time that it must remain in some measure accumulated within them.

Among the facts which appear favourable to the Huygenian theory, we must first enumerate the uniformity of the velocity of light in any one medium, under all circumstances that have hitherto been observed; since it is a fundamental law of this system, while it cannot easily be explained from any probable mode of operation of repulsive forces; and in the second place, the precise agreement of the hypothesis of Huygens, respecting spheroidal undulations, with the phenomena of extraordinary refraction, and the immediate connexion, which we have pointed out, in a former article,* between this hypothesis and the simplest possible supposition respecting the constitution of a stratified medium; while on the other hand we imagine that we have said enough to make it evident, that neither Mr. Laplace, nor his critic, has succeeded in deducing any explanation of the facts from the ordinary laws of accelerative forces. The rectilinear motion of the light, passing near a material substance, has often been adduced as an argument in favour of the projectile system; but we are inclined to class the phenomena, which occur under such circumstances, with those which are most conveniently explained by the undulatory theory. The dimensions of the shadow of a hair, as observed by Newton and other authors, are such as to show undeniably, that light passing at a distance of one *tenth* of an inch, or more, from an opaque body, is deflected in its course, and at length dissipated into the surrounding space; now it is contrary to all probability, and even to direct experiment, to maintain, that any repulsive force can act on light at such a distance: indeed, if we judged of the extent of the supposed repulsive force by that which is exhibited on the approach of two hard bodies, we should not expect it to act beyond the distance of one *ten thousandth* of an inch. It has also been ascertained, that the phenomena of light, inflected in this manner, are totally independent of the refractive density of the bodies concerned, which they could not well be, if the same forces were employed in them as are the immediate agents in reflection and refraction. We do not know that any

* Supra, No. XII.

attempt has been made to assign the precise magnitude of the addition to the breadth of the shadow from this diffraction, at different distances, but we believe it will always be nearly represented by $\frac{1}{300} x^{\cdot 7}$, x being the distance from the object in inches. There are also many other cases in which it is absolutely necessary to suppose such a diffraction in order to reconcile the phenomena with the results of calculation.

Having thus endeavoured to state the arguments on both sides, in the most impartial manner, we must leave our readers to satisfy themselves, if they can, with the theory to which they may be most inclined: for ourselves, we confess that we are compelled to remain for the present undecided, and we can only look forwards for further information to the discoveries which may result from future experiments.

Abstract of Memoirs read to the Institute from the 7th March, 1808, to the 27th February, 1809. By MM. GAY LUSSAC and THÉNARD.

The principal part of these eight memoirs relates to the brilliant and important discoveries with which our countryman Mr. Davy has enriched the science of chemistry. It is true that the authors have confined themselves principally to the relation of their own experiments, many of which are certainly in some degree original, and possess great merit: but in other instances, they have not been so accurate in avoiding the appearance of laying claim to the discoveries of another, as might have been consistent with perfect liberality of sentiment. We are also sorry to observe in various parts of these volumes that the obligations of several authors to the theories and experiments of Mr. Dalton have not been so distinctly acknowledged, as candour might have required. We have heard, indeed, that the successes of the chemists of other nations have sometimes been held up as reproaches to the members of the National Institute by a powerful protector; and that these reproaches have even been accompanied by threats of abandonment. Supposing this to have really happened, we can readily make allowances for the substantial causes, which may have contributed to make the sight a little dim in reading across the Channel.

But we are not disposed to be quite so indulgent to that imperfection of the organs, which obscures all objects that are merely seen across the Tweed. We think that no impartial judge, exempt from the influence of an Antianglican spirit, would have professed to believe that MM. Thénard and Gay Lussac have established "most convincingly, that the new metals are not simple substances, but really compounds of the several bases with hydrogen." Mr. Davy has most abundantly confuted this rash and ill-supported opinion, derived from the accidental result of a single experiment, and incapable of being reconciled with the opinions professed even by its authors in other memoirs. And who in this island has a right to expect that his cursory adoption of a foreign hypothesis shall be put in competition with the deliberate judgment of a chemical philosopher like Mr. Davy?—a man whose candour is equal to his ingenuity, and whose uncommon talents have been seconded by the most ardent zeal for the acquirement of knowledge, and have been crowned by a good fortune commensurate to his exertions and his opportunities! The coherence of the analogical argument, which is offered in support of the hypothesis of the French chemists, is well calculated to accompany the modesty with which the truth of the opinion is asserted. "Every compound must have the intermediate density of its distinct ingredients." This observation is in no sense universally true: but let it pass. Now "the specific gravity of the alkaline metals is far less than that of the substances from which they are derived." If these words convey any ideas at all, they are certainly not such as are applicable to the very simple case in question. Potassium is very light: when combined with fixed oxygen, it forms potass, which is heavier than potassium, but may, for aught we know, be far lighter than fixed oxygen; nor, if it were otherwise, would the case be absolutely unique.

The difficulty, which has given rise to this unwarrantable opinion respecting the metals of the alkalis, originated in an experiment on the decomposition of ammonia, in which a considerable portion of hydrogen appeared to be set at liberty. But according to Mr. Davy's latest repetition of this experi-

ment, in a tube bored out of solid platina, there seems to be very little mystery in the process: the hydrogen and nitrogen are both recovered in their proper proportions, except that there is rather a deficiency of hydrogen than an excess, this substance appearing partly to enter into combination with the platina. If, however, in other circumstances, the results should appear to be more complicated, we shall be much more willing to admit Mr. Davy's modest conjecture respecting the constitution of nitrogen, than the singular hypothesis of the French chemists respecting that of potassium.

In the fluoboric gas, discovered by the author of these memoirs, there seems to be a singular exception to Mr. Dalton's general laws of hygrometry; for this substance does not appear to be capable of containing any aqueous vapour; while Mr. Dalton maintains that the quantity of aqueous vapour, which may be present in any space, is nearly independent of the nature of the gas that occupies it. The contradiction is, however, perhaps more apparent than real, since the condensation of the vapour is owing to the formation of a new substance, in consequence of the strong chemical attraction of the gas for water; and this new fluid, which is a most corrosive acid, follows its own particular laws with respect to evaporation, being extremely little disposed to assume a gaseous form.

Experiments on the Propagation of Sound through solid Bodies, and through the Air contained in very long Pipes. By Mr. Biot.

From a number of very accurate experiments on the transmission of the sound of a bell, fixed to one end of a series of pipes of cast iron, 3121 feet in length, Mr. Biot has inferred that its velocity, in passing through the substance of the pipes, was between 10 and 11 times as great as in the air which they contained. A whisper was easily heard at night through the whole of this length, but in the day the words spoken by the loudest voice could not be distinctly understood at a much shorter distance. In speaking through the whole of the pipe,

it was observed that several repetitions of an echo returned to the speaker at intervals of half a second each. This circumstance is not explained: perhaps it arose from some accidental projections within the pipes; but it is singular that these should have been at equal distances. This difficulty may indeed be avoided by attributing the echoes to the return of the sound from the opposite extremity of the whole pipe, through its substance, which, by the former observation, ought to have occupied exactly .52″: but on this supposition an equal number of repetitions should have been heard at the other end of the pipe; while in fact one sound only was heard, and this was conveyed through the air.

Mr. Biot's determination of the velocity of the transmission of sound, through the substance of the pipes, is so far interesting, as it tends to the confirmation of other experiments, which are in their nature susceptible of more accuracy: but the precise results, which he has obtained, are of no value whatever. The ends of the separate pieces of pipe, the shoulders of which were screwed together, with the interposition of wadding, must have materially retarded the transmission of the sounds, by the increase of their bulk, in the same manner as any dilatation or contraction of a cylindrical cavity, for instance that of a chimney pipe, retards the vibration of the medium contained in it. Mr. Chladni's experiments, which are exempt from this cause of error, make the velocity 16 or 17 times as great in iron as in air.

Before we take our leave of the contents of these volumes, and of the remarks which have been made on them, we must submit to our readers one more specimen of inaccuracy, which appears to us to be sufficient of itself to determine the degree of confidence which ought to be placed in these remarks. "Chladni," says the critic, "had assigned the celerity of vibration through iron and glass at 17,500 feet in a second; and *Leslie had shown,* in one of the curious notes annexed to his book on heat, that through a fir board the velocity of impulsion, which *he proved* to be the same as that of vibration, *is* 17,300 *feet in a second.*" Now, having referred to Professor Leslie's note relating to his experiment, p. 519, we find that the height of the column, measuring the elasticity of fir, is there

calculated to be only "671,625 feet," which corresponds to a velocity of 4640 feet in a second: and in the text it is asserted, probably on the ground of an earlier and still more hasty estimate, that motion is conveyed through deal "with 5½ times the velocity of sound," that is, with a velocity of about 6220 feet in a second. It appears, therefore, that "the velocity of impulsion," as really calculated by Professor Leslie, is less than *one-third* of that which Chladni had assigned from more direct experiments. Where then can the critic have found a number approaching so much more nearly to the truth? We can only answer, that *we* have found it by looking into the index of Dr. Young's Natural Philosophy, for the article "sound in wood:" we are there referred to a passage in which it is said expressly, vol. ii. p. 266, that "according to the elasticity of fir, as inferred *from an experiment* of Mr. Leslie, the velocity of an impulse should be 17,300:" and it appears, from the same volume, p. 49, that Dr. Young's calculation was the result of a series of original investigations, applied, in this as well as in several other cases, to circumstances which had not before been sufficiently examined. Perhaps the critic had long ago consulted the same index, and found the same passages; perhaps, considering it as of no importance to the establishment of a point of calculation, to recollect from what work it was borrowed, he has *unintentionally* substituted the name of a Tyrian for that of a Trojan, *nullo discrimine*. We confess, however, that we think a censor ought to be more rigidly correct.

We have perhaps detained our readers too long with the correction of errors which may be thought incapable of misleading those who reason at all for themselves: but the work, in which they are contained, has long assumed an air of authority, which may have imposed on the timid, and satisfied the superficial student; and it was time that some attempt should be made to reduce its pretensions to their natural level. We trust that our remarks may have a prospective as well as a retrospective effect; and that, without being again obliged to undertake the disagreeable task of controversial discussion, we shall have inspired the candid lovers of science with a salutary distrust,

which will prevent their acceding unguardedly to all the dogmas that may hereafter be dictatorially proclaimed through the same channel, in conformity with the system which seems to have been adopted, of the uniform discouragement of all domestic pretensions to scientific merit, beyond the limits of a particular school.

No. XIV.

REVIEW OF

MALUS, BIOT, SEEBECK, AND BREWSTER ON LIGHT.*

From the Quarterly Review for April, 1814, vol. xi., p. 42.

THE intimate connexion of the subjects of these works with each other renders it unnecessary to make any apology for including our account of them in one article; since the greater number of the observations which they contain have arisen more or less immediately from the prosecution in the different parts of Europe, of the important discoveries of Mr. Malus, respecting the properties exhibited by light which has been subjected to oblique reflection or refraction. Of these discoveries we have already given some account (*supra*, p. 247); and the honourable testimonials of public approbation, which their author has since received, in particular from the Royal Society of London, as well as from the Institute of France, sufficiently show that our estimate of his merits was not exaggerated. Most unfortunately for the sciences, his career has been cut short by an early death, in the midst of his researches and improvements; but this event did not take place, as Dr. Brewster seems to imagine, so immediately after the adjudication of Count Rumford's medal, as to have rendered him incapable of being informed of the honour that was conferred on him, and of appreciating its value.

* 1. THÉORIE DE LA DOUBLE RÉFRACTION DE LA LUMIÈRE. Par E. L. MALUS. 4to. Paris, 1810. pp. 302; with 8 Plates.

2. MÉMOIRE SUR DE NOUVEAUX RAPPORTS ENTRE LA RÉFLEXION ET LA POLARISATION DE LA LUMIÈRE. Par M. BIOT. Lu à l'Institut le 1 Juin, 1812. pp. 152; with 1 Plate.

3. VERSUCHE UEBER SPIEGELUNG UND BRECHUNG. [EXPERIMENTS ON THE REFLECTION AND REFRACTION OF LIGHT.] By Dr. SEEBECK.

4. A TREATISE ON NEW PHILOSOPHICAL INSTRUMENTS, WITH EXPERIMENTS ON LIGHT AND COLOURS. By DAVID BREWSTER, LL.D. 8vo. Edinburgh, 1813. pp. 442; with 12 Plates.

In the present work of Mr. Malus, there is less of absolute novelty than of minute and interesting research, upon a point respecting which some doubt was perhaps entertained, by those who were not sufficiently acquainted with the few, but satisfactory, experiments relating to it, which before had been made in this country; that is, upon the perfect accuracy of the Huygenian laws of refraction in the Iceland crystal, and other doubling substances; which, considered in itself, he thinks "one of the finest discoveries of this celebrated geometrician."

"Newton," he observes, "was acquainted with the investigations of Huygens; yet he attempted to substitute for the Huygenian law another, apparently more simple, but absolutely contrary to the phenomena, as Mr. Haüy first observed and demonstrated. It is difficult to explain the disregard that Newton showed, in this case, to a law which Huygens had declared to be conformable to his experiments."

"Wollaston has examined the refractive power of the crystal, by a very ingenious method of his own invention, and has shown that the law is perfectly true in all cases of rays passing in the direction of any surface of the crystal. He is the *first* that, after the oblivion of a century, *thought* of verifying, by direct experiments, a law which Huygens had considered as incontestable, and which Newton had rejected without examination."

It may not, however, be altogether superfluous to observe, that as Dr. Wollaston's experiments seem to have led to Mr. Malus's researches and discoveries, so Dr. Wollaston's *thoughts* were in all probability directed to the Huygenian theory by an *earlier* paper published in the same volume of the Philosophical Transactions with his own, in which it is asserted, almost in the terms that Mr. Malus has employed, that Newton, "without attempting to deduce from his own system any explanation of the more universal and striking effects of doubling spars, has omitted to observe that Huygens's most elegant and ingenious theory *perfectly accords* with these general effects in all particulars."* Ph. Tr. 1802. 45.† In short, whoever reads

* In a note from Dr. Wollaston, dated August 26th, 1801, addressed to Dr. Young, and referring to his Bakerian Lecture, No. VII., which had been submitted to him, he says: "I like your Bakerian very much, but I cannot say that I have yet inserted the undulatory doctrine into my creed, and it may be some time before I repeat it with fluency." He was constitutionally reluctant to acknowledge, or to publish, any theory or observation, of the entire correctness of which he was not himself fully satisfied.—*Note by the Editor.*

† Supra, No. VII., p. 166.

the account which Huygens gives of his own examination of these substances, can scarcely fail to be convinced that this law must be extremely near the truth: Dr. Wollaston's experiments afforded additional evidence of its accuracy; and Mr. Malus, having diversified his calculations and observations in a still greater variety of forms, has left nothing further to be desired for the complete re-establishment of this remarkable result of hypothetical theory, the ground-work of which is still by no means unexceptional, notwithstanding the wonderful simplicity to which, as we have shown on a former occasion (*supra*, p. 228), it is capable of being easily reduced.

Mr. Malus has prefixed to his experimental investigations an analytical treatise on optical phenomena in general, which will probably be thought, by most English readers, unnecessarily intricate, and which does not appear to contain any material novelty. He has examined the forms of the principal refracting substances by means of a reflective goniometer, resembling Dr. Wollaston's; and he has taken the mean of a considerable number of successive repetitions of the measurement. For the angle of the Iceland spar he obtains in this manner, 74° 55′ 2″½; and contents himself with 74° 55′, which is precisely Dr. Wollaston's measure: for the indices of refraction he gives 1.6543 and 1.4833, instead of Dr. Wollaston's 1.657 and 1.488, although the experiments on some specimens go as far as 1.658. For quartz crystal we have 1.5582 and 1.5484; for the sulfate of barita, 1.6468 and 1.6352; and for the arragonite, another form of the carbonate of lime, which some have suspected to contain strontia, 1.6931 and 1.5348. From the laws of extraordinary reflection within a crystal of doubling spar, Mr. Malus has very ingeniously deduced an explanation of a reduplication of images, long since observed by Martin, in particular specimens, which appear to have been interrupted by fissures of such a nature, as to be capable of producing a subdivision of the rays, like that which takes place in the internal reflections: the colours observable in these images he refers to the thickness of the fissures, although it seems at least equally probable that they are more nearly related to the colours of crystallized substances, since described by Biot and others.

Mr. Malus's calculations of the particular cases of refraction are founded on the Huygenian method of drawing a tangent plane to the supposed spheroid, from a point in the surrounding medium, at which the supposed original undulation would have arrived while the spheroid is generated. The steps of this mode of calculation are, however, extremely intricate; and it has occurred to us that the problem may be solved in a much more simple manner by equating the velocities with which the supposed undulations must advance upon the common surface of the respective mediums—a condition which is obviously sufficient for the determination of the angular directions of the actual undulations—just as the velocity with which a bird swims on the surface of a piece of water, is sufficient for determining the direction of the wave which follows it. Considering the velocity of the advance of the undulation with regard to the spheroid, it must evidently be identical with the velocity of increase or decrease of the perpendicular to the circumference of the section cut off by the refracting surface; and with regard to the surrounding space, it must be to the direct velocity, as the radius to the sine of the angle of incidence or refraction in that space. Hence, if r be the index of the greatest refractive density of the substance, s the sine of incidence or refraction without the crystal, x the semiaxis of the spheroid, and y the perpendicular falling from the point of incidence on the conjugate diameter of the section, we have the equation $r\dot{x} : s = \dot{y}$; which determines the physical conditions of the problem, and reduces it to a mathematical investigation.

Now if the ratio of the greatest and least refractive densities, or of the equatorial diameter of the spheroid to the axis, be that of n to 1, and the tangent of the angle formed by the axis with the refracting surface, p, it may readily be inferred, from comparing the ordinates of the ellipsis with those of the inscribed circle, and from the properties of similar triangles, that the semidiameter parallel to the given surface will be $n\sqrt{\frac{1+pp}{nn+pp}}\,x$, the tangent of the angle formed by the conjugate semidiameter with the axis, $nn : p$, and the length of this semidiameter $\sqrt{\frac{p^2+n^4}{nn+pp}}\,x$. From the known equality of all paral-

lelograms described about an ellipsis, we have, for the perpendicular falling from the end of this semidiameter on the former, $\sqrt{\frac{nn+pp}{1+pp}}\,x$; and taking the difference of the squares, $\frac{p\,(nn-1)\,x}{\sqrt{(nn+pp)}\,\sqrt{(1+pp)}}$, for its distance from the centre; and for the sine of the included angle $\frac{p\,(nn-1)}{\sqrt{(p^2+n^4)}\,\sqrt{(1+p^2)}} = t$. The perpendicular falling from the same point on the axis will be $\frac{nnx}{\sqrt{(nn+pp)}} = u$, and its distance from the centre, $\frac{px}{\sqrt{(nn+pp)}} = v$.

Proceeding now to the section formed by the given refracting surface, let q be the cotangent of the angle comprehended by its lesser axis and the plane of the ray's motion without the crystal, and let z be the distance of its centre from that of the spheroid: we shall then have, for the lesser semiaxis of the section, $\sqrt{\left(\frac{p^2+n^4}{nn+pp}\,x^2-z^2\right)}$, reduced in the ratio of the conjugate diameters of the spheroid, that is, $\sqrt{\left(\frac{p^2+n^4}{nn+pp}\,x^2-z^2\right)} \times n\sqrt{\frac{1+pp}{p^2+n^4}}$. The ratio of the axes of the section, from the known similarity of parallel sections of a spheroid, will be that of $n\sqrt{\frac{1+pp}{nn+pp}}\,x$ to nx; or if we call this the ratio of 1 to m, we have $m = \sqrt{\frac{nn+pp}{1+pp}}$. Hence, in order to find y, we must substitute these values in the expression for the perpendicular $\sqrt{\frac{nn+pp}{1+pp}}\,x$, whence we have $y = \sqrt{\frac{mm+qq}{1+qq}}\,\sqrt{\left(\frac{p^2+n^4}{nn+pp}\,x^2-z^2\right)} \times n\sqrt{\frac{1+pp}{p^2+n^4}}$; and taking the fluxion, $r\dot{x} : s = \dot{y} = \sqrt{\frac{mm+qq}{1+qq}}$, $\frac{p^2+n^4}{nn+pp}\,x\dot{x}n\,\sqrt{\frac{1+pp}{p^2+n^4}} : \sqrt{\left(\frac{p^2+n^4}{pp+nn}\,x^2-z^2\right)}$, z being supposed to remain constant; consequently $\sqrt{\left(\frac{p^2+n^4}{pp+nn}\,x^2-z^2\right)} = \frac{s}{r}\sqrt{\frac{mm+qq}{1+qq}}\cdot\frac{p^2+n^4}{nn+pp}\,x\,n\,\sqrt{\frac{1+pp}{p^2+n^4}}$; and the semiaxis of the section, $n\sqrt{\frac{1+pp}{p^2+n^4}}\,\sqrt{\left(\frac{p^2+n^4}{nn+pp}\,x^2-z^2\right)} = \frac{s}{r}\sqrt{\frac{mm+qq}{1+qq}}\,n^2\,x\,\frac{1+pp}{nn+pp}$,

and the semidiameter of the section ending at the point of incidence, $\frac{s}{r}\sqrt{\frac{q^2+m^4}{1+qq}}\,n^2 x\,\frac{1+pp}{nn+pp}$: whence it is obvious that this semidiameter, which may be considered as an ordinate in an elliptic section passing through the centre of the spheroid, is proportional to the sine of incidence, as Huygens has demonstrated; and it will appear that the tangent of the angle formed by the plane of this section with the plane of incidence is $\frac{p\,(nn-1)}{nn+pp}\cdot\frac{mm}{\sqrt{(q^2+m^4}}$.

But in order to determine more directly the inclination of the ray within the crystal, we must find u, the perpendicular falling from the point of incidence on the lesser semiaxis, before expressed by $\frac{nnx}{\sqrt{(nn+pp)}}$ and now $=\frac{mmnns}{r\sqrt{(1+qq)}}\,x\,\frac{1+pp}{nn+pp}$, and its distance from the centre of the section $v=\frac{qnns}{r\sqrt{(1+qq)}}x\,\frac{1+pp}{nn+pp}$, and from the point nearest to the centre of the spheroid, $tz-v$; whence the distance of the point of incidence from this last point must be $\sqrt{([tz-v]^2+u^2)}$: and adding to the square of this that of the perpendicular falling on the section from the centre of the spheroid, or $z^2-t^2z^2$, we have $\sqrt{(v^2+u^2+z^2-2tvz)}$ for the semidiameter at the point of incidence, expressing the velocity: and the sine of the corresponding angle of incidence or refraction will be $\sqrt{([tz-v]^2+u^2)}$ divided by this radius, while that of the inclination of the plane to the axis will be $u:\sqrt{([tz-v]^2+u^2)}$; z being $=x\sqrt{\left(\frac{p^2+n^4}{nn+pp}-\frac{ss}{rr}\cdot\frac{mm+qq}{1+qq}\cdot\frac{p^2+n^4}{nn+pp}\,n^2\,\frac{1+pp}{nn+pp}\right)}$. It is also evident that the velocity, reduced to the direction of a perpendicular to the surface, will vary as $\frac{z}{x}\sqrt{(1-t^2)}$.

These expressions may be much simplified by further reduction, especially where they are to be applied to surfaces either parallel or perpendicular to the axis: since in these cases $p=0$ and $m=n$, and $p=\infty$ and $m=1$ respectively, and $t=0$ in both. Hence, in the first case, $z=nx\sqrt{\left(1-\frac{nn+qq}{1+qq}\cdot\frac{ss}{rr}\right)}$, $u=\frac{nnsx}{r\,(1+qq)}$, $v=\frac{qsx}{r\sqrt{(1+qq)}}$ and $\sqrt{(u^2+v^2)}=\frac{sx}{r}$

$\sqrt{\frac{n^4 + q^2}{1 + qq}}$, whence the sine of the angle may be found, dividing it by $\sqrt{(u^2 + v^2 + z^2)}$; and the tangent will be $\frac{\sqrt{(u^2 + v^2)}}{z} = \frac{s}{n}\sqrt{\frac{n^4 + q^2}{1 + qq}} : \sqrt{\left(r^2 - \frac{nn + qq}{1 + qq}s^2\right)}$; and in the second case, $z = x\sqrt{\left(1 - n^2\frac{ss}{rr}\right)}$, $u = \frac{n^2sx}{r\sqrt{(1 + qq)}}$, $v = \frac{n^2qsx}{r\sqrt{(1 + qq)}}$, and $\frac{\sqrt{(u^2 + v^2)}}{z} = \frac{nns}{\sqrt{(r^2 - n^2s^2)}}$.*

It is not merely with a view of exhibiting a more convenient mode of solving a problem which Mr. Malus had solved before, that we have introduced this calculation, but in order to apply it to the explanation, which we shall attempt to give, of the very interesting phenomena described at large in Mr. Biot's memoir.

Mr. Arago had discovered, in 1811, that polarised light was resolved, by passing through thin plates of mica or sulfate of lime, or thicker plates of rock crystal, and of some kinds of flint glass, into two portions differently coloured.† Mr. Biot has experimentally investigated the law according to which these phenomena take place, and has reduced the results of his experiments into such a form as to enable us to calculate from them what colours will be exhibited by a plate of sulfate of lime, of a given thickness, and in a given situation with respect to the incident light.

The axis of the crystals of sulfate of lime is, either accurately or very nearly, in the plane of the plates which they afford, and makes an angle of 16° 13′ with one of the natural lines of fracture of the plates; while that of rock crystal is nearly parallel to the longitudinal surfaces of the crystal. Mr. Biot's method of exhibiting the colours in question is to take a thin and smooth plate of sulfate of lime, or Muscovy talc, or a well-polished plate of rock crystal, cut as thin as possible, which affords no appearance of colour in the open air, except when some of the

* The same investigation, very slightly modified, is given in the article Chromatics, Section XIV., which immediately follows in this volume. The substance of that given by Malus, founded upon the principle of least action, may be seen in Sir J. Herschel's well-known Treatise on Light in the Encyclopædia Metropolitana, Art. 788.—*Note by the Editor.*

† The Memoir announcing this important discovery was read to the Institut in 1811, and published in the volume of Memoirs for that year.—*Note by the Editor.*

incident light has been polarized by reflection from the blue atmosphere, and to place it horizontally on a black substance; then, allowing the white light of the clouds to fall on it, at an inclination of about 35°, to receive this light when reflected from it, on a black glass, making an equal angle with the reflected rays, in a plane perpendicular to the first plane of reflection; so that the plate may be visible by reflection in the black glass. In this manner the plate appears to be very brilliantly illuminated by the light of the colour which it is calculated to exhibit: when its axis coincides with the plane of incidence, no colour is visible; and the appearance becomes most distinct when the axis makes an angle of 45° with that plane. In this situation of the axis Mr. Biot finds that the colour reflected by talc and by rock crystal is precisely the same as if the incidence were perpendicular, and the same as is transmitted by the extraordinary refraction; while the light transmitted by the ordinary refraction exhibits the complementary colour, as in the case of the ordinary colours of thin plates: these transmitted colours being separable, as Mr. Arago had found, by means of any doubly refractive substance, or by oblique reflection. In Mr. Biot's arrangement, the light reflected from the upper surface of the plate is polarized according to the general law, and is, therefore, not reflected by the black glass, but absorbed; and the same is true of the light reflected from the lower surface of the plate, and then transmitted back by the ordinary refraction: but that which has been transmitted [back from] the lower surface by the extraordinary refraction [not to it, as Mr. Biot's words imply], has acquired a contrary character: and when it arrives at the black glass, it is partially reflected. On the other hand, a black glass, of which the plane of incidence coincides with that of the plate, reflects the complementary tint afforded by the light which had been reflected by the lower surface of the plate, and transmitted back by the ordinary refraction, but exhibits the colour more faintly, because it is mixed with the white light reflected from the upper surface. A similar arrangement may also be very conveniently applied to the observation of the colours of natural bodies, independently of the glare occasioned by their superficial reflection.

The colours dependent on the extraordinary refraction Mr. Biot found to agree exactly with the colours of thin plates of glass as seen by reflection, and those which are derived from the ordinary refraction with the colours seen by transmission in the Newtonian experiments, supposing the thickness of the plate to be reduced in the ratio of 360 to 1; this ratio being constant for the same specimen of the talc, although the number varied in different specimens from 333 to 395. For mica, it appeared to be 450, but was liable to still greater variation: for rock crystal, it was exactly 360, at least in several plates cut out of the same piece. The measurements of the thickness of the plates were executed with the greatest care by Mr. Cauchoix's spherometer, which appears to be capable of great precision, although the pressure exerted by a fine screw, which is the immediate instrument of examination, must be a cause of considerable uncertainty, where the objects to be measured are extremely minute.

Mr. Biot observed that when the axis of the crystal approached to the plane of incidence, the colours ascended in the scale of Newton's measures, as if the thickness were diminshed; and that they descended when the plate was turned in a contrary direction. The difference thus produced appeared to be greater in plates of rock crystal and of mica than in those of talc; but the comparative measures have not been detailed; and it may be remarked that the greater thickness of the plates of rock crystal employed may possibly have made the difference more apparent. When the axis made an angle of 45° with the plane of incidence, the change of the inclination of the incident light had no effect on the colour exhibited either by talc or by rock crystal; but mica, probably from the oblique situation of the axis of refraction, did not observe the same law. Mr. Biot has expressed the thickness corresponding to the tint, exhibited under these different circumstances, by the formula $1 + (.065\ \varsigma^2\ H - .195 \cdot \varsigma 2\ H)\ s^2$; while, in another series of experiments, the coefficients appeared to be .00959 and .1428; H being the angle formed by the axis with the plane of incidence, and s the sine of the angle of incidence; so that the greatest possible variation must have been from .87 to 1.26, or from .867 to 1.152.

Mr. Biot has also improved Mr. Malus's expressions for the intensity of the light under different circumstances; but as the colour is wholly independent of the intensity, we omit to mention these expressions more particularly.

This intricate and laborious investigation appears to have been conducted with much patience, and with minute attention to the strictest accuracy; nor does the present memoir by any means exhaust the whole of the experiments which Mr. Biot has promised to the public. Dr. Brewster has remarked that he has "the undivided merit of having generalized the facts," and of having "discovered the law of these remarkable phenomena." This "law," however, is merely an expression of the facts considered as insulated from all others; and not an explanation by which they are reduced to an analogy with any more extensive class of phenomena; and we have no doubt that the surprise of these gentlemen will be as great as our own satisfaction in finding that they are perfectly reduceable, like all other cases of *recurrent* colours, to the general laws of the interference of light, which have been established in this country, and of which we have given an account in our sixth number (*supra*, p. 251); and that all their apparent intricacies and capricious variations are only the necessary consequences of the simplest application of these laws. They are, in fact, merely varieties of the colours of "mixed plates," in which the appearances are found to resemble the colours of simple thin plates, when the thickness is increased in the same proportion, as the difference of the refractive densities is less than twice the whole density: the colours exhibited by "direct transmission," corresponding to the colours of thin plates seen by reflection, and to the extraordinary refraction of the crystalline substances, and the colours of mixed plates exhibited by "indirect light" to the colours transmitted through common thin plates, and to those produced by the ordinary refraction of the polarizing substances.* The measures which Mr. Biot has obtained differ much less from the calculation derived from these principles only, than

* This important step in the theory, though correct in principle, is incomplete, inasmuch as the effects of polarization in modifying the phenomena of interference, had not yet been ascertained: they were announced by Arago and Fresnel in the tenth volume of the 'Annales de Chimie' for 1819.—*Note by the Editor.*

they differ among themselves; and we cannot help thinking such a coincidence sufficient to remove all doubts (if any existed) of the universality of the law on which that calculation is founded; notwithstanding the difficulty of explaining the production of the different series of colours by the different refractions. (See Qu. Rev., No. XVII. p. 124.)*

In the first place, it appears from Mr. Malus's experiment, that the extraordinary and ordinary refractive densities of the rock crystal, in a plane perpendicular to the axis, are in the ratio of 159 to 160; consequently the difference of the times is to twice the whole time in the ordinary refraction as 1 to 320, and to the time in a plate of glass of which the refractive density is 1.55, as 1 to 318. In Mr. Biot's experiments on this substance, the proportion of the thicknesses appeared to be 1 to 360, while in the sulfate of lime, the number varied from 333 to 395: and it must be observed that any accidental irregularities, or foreign substances adhering to the plate, would tend, in Mr. Biot's mode of measurement, to make the thickness appear greater: while, on the other hand, an error of a single unit in the third place of decimals of the index of refractive density, as determined by Mr. Malus, would be sufficient to make the coincidence perfect: and a greater degree of accuracy can scarcely be expected in experiments of this kind.

We have next to inquire what must be the effect of the obliquity of the incident light according to the general law of periodical colours; and we shall here find the agreement of the experiments with the theory equally striking. We must compare the excesses of the times occupied in the transmission of light by the respective refractions, above the time required for its simple reflection from a point in the upper surface, exactly opposite to the respective point of reflection in the lower; and the difference between these excesses will give the interval required for determining the colour. Calling the thickness unity, and the sine of incidence s, the excess for the ordinary refraction will be represented by the time within the plate, which is as the secant of refraction, diminished by the difference of the times without the plate, which is as its tangent,

* In a Review of Dr. Young's Introduction to Medical Literature.—*Note by the Editor.*

and as the sine of incidence jointly, (see fig. 130,) or by r : $\sqrt{(1 - \frac{ss}{rr})} - ss : r\sqrt{(1 - \frac{ss}{rr})} = \sqrt{(r^2 - s^2)}$; and for the extraordinary refraction, when the axis is parallel to the surface, the former part will be inversely as $\frac{z}{x}$, and will be expressed by $r : n\sqrt{(1 - \frac{ss.}{rr}\frac{nn + qq}{1 + qq})}$ and the latter by $\frac{ss}{n}$ $\sqrt{\frac{n^4 + q^2}{1 + qq}} : \sqrt{(r^2 - \frac{nn + qq}{1 + qq}s^2)}$, whence the whole becomes $(r^2$ $- \sqrt{\frac{n^4 + q^2}{1 + qq}}\, s^2 : n\sqrt{(r^2 - \frac{nn + qq}{1 + qq}s^2)}$. Now since, in the substances which we are considering, n is little more than 1, we may put $n = 1 + l$, $n^2 = 1 + 2l$, and $n^4 = 1 + 4l$; then $\sqrt{\frac{n^4 + q^2}{1 + qq}} = \sqrt{(1 + \frac{4l}{1 + qq})} = 1 + \frac{2l}{1 + qq}$; which will also be the value of $\frac{nn + qq}{1 + qq}$; and if for $\frac{1}{1 + qq}$ we write k^2, the excess will become $\frac{rr - (1 + 2kkl)\, ss}{n\sqrt{(rr - (1 + 2kkl)\, ss}} = \sqrt{(r^2 - (1 + 2k^2l)\, s^2)} : n$. Now the difference between $\sqrt{(r^2 - s^2)}$ and $\sqrt{(r^2 - (1 + 2k^2l)\, s^2)}$ is $\frac{k\, k\, lss}{\sqrt{(rr - ss)}}$; and the difference between the latter root, and the same quantity divided by n, is $l\sqrt{(r^2 - (1 + 2k^2 l s^2)}$, or very nearly $l\sqrt{(r^2 - s^2)} = l\frac{rr - ss}{\sqrt{(rr - ss)}}$, and the sum of these differences is $l\frac{rr - (1 - hk)\, ss}{\sqrt{(rr - ss)}}$, or if $1 - k^2 = h^2$, $l\frac{rr - hh\, ss}{\sqrt{(rr - ss)}}$, h being the cosine of the inclination of the plane of incidence to the axis; nor will the result be sensibly affected by taking into account the deviation of the refracted ray from this plane in oblique situations.

This expression will be found to include all the effects of a change of inclination observed by Mr. Biot, and to agree sufficiently well with the formula which he has deduced from his measurements. When the light falls perpendicularly on the surface, $s = 0$, and the difference becomes lr; when its obliquity is the utmost possible, s being 1, the expression becomes $l\frac{rr - hh}{\sqrt{(rr - 1)}}$, and its value varies in the ratio of r^2 to

r^2-1, according to the position of the axis. Thus in the sulfate of lime, r being 1.525, according to Dr. Wollaston's table, the utmost possible variation is in the ratio of 2.326 to 1.326, and the equivalent thickness for perpendicular rays being called 1, the extremes will become .755 and 1.325, instead of .87, and 1.26 or 1.152, which are the results of Mr. Biot's different formulas; and the difference between these is as great as the variation of the first of them from our calculation. With respect to the singular fact of the indifference of the angle of incidence, when the inclination of the plane of incidence to the axis is 45°, our expression agrees exactly with Mr. Biot's observations: for when $h^2 = \frac{1}{2}$, $\frac{rr - \frac{1}{2}ss}{\sqrt{(rr-ss)}} = r$, very nearly: thus if $s = 1$, it only becomes 1.586 instead of 1.525, and does not vary sensibly while s remains small.

In a similar manner the result may be determined for any other relative situations of the axis and the refracting surface: if, for instance, they are perpendicular to each other, $\frac{z}{x}$ being $\sqrt{(1 - n^2 \frac{ss}{rr})}$, and the tangent of refraction $\frac{nns}{\sqrt{(rr-nnss)}}$, the expression for the excess of time becomes $r : \sqrt{(1 - n^2 \frac{ss}{rr})} - \frac{nns}{\sqrt{(rr-nnss)}} = \sqrt{(r^2 - n^2 s^2)}$, while the excess for the ordinary refraction is $\sqrt{(r^2 - s^2)}$ as before; and the difference becomes $\frac{lss}{\sqrt{(rr-ss)}}$, which vanishes with the angle of incidence, and becomes ultimately $\frac{l}{\sqrt{rr+1}}$. We cannot help thinking ourselves justified in looking forwards to a perfect coincidence between this formula and the promised experiments of Mr. Biot on substances placed in these circumstances. We understand that Dr. Brewster has lately made some observations of a nature nearly similar; but we doubt whether he has determined the refractive powers of his crystals with sufficient accuracy to allow of the application of our calculations with perfect precision.

A singular confirmation of the mode of explaining the colours of thin plates, which we have adopted, is afforded by the expe-

riments of Mr. Arago, who found that the light forming the transmitted rings appeared to be polarized in the same direction with the reflected light, while the rest of the transmitted light was polarized in a contrary direction. It is a necessary assumption in the theory of periodical colours, that the rings seen by transmission actually depend on light twice reflected within the plate. and which must therefore be polarized like the rest of the reflected light; although without these experiments of Mr. Arago, it would have been difficult to obtain so direct a demonstration of the fact.

The colours exhibited by thick pieces of rock crystal, cut as in Mr. Biot's unpublished experiments, perpendicularly to the axis, might be expected to afford some explanation of those which Dr. Seebeck has observed in large cubes or cylinders of glass placed between two oblique reflecting surfaces, or between two piles composed of thirty pieces of glass each, which produced the effect of complete polarization on light transmitted at the appropriate angle. If, however, Dr. Seebeck's observations are correct, the analogy can be only superficial; for the effects of these pieces of glass seem to depend on their entire magnitude and outward form, without any particular relation to an axis of extraordinary refraction. Thus in the perpendicular transmission of the polarized light through any points in the diagonals of the surfaces of the cubes, or in the diameters parallel to their sides, the rays of different colours appeared to be differently affected according to the part of the glass on which they fell, and to exhibit one or the other only of the two images, which would have been visible through a piece of doubling spar, if the glass had not been interposed; so that when the whole cube was viewed at once under these circumstances, it afforded an appearance of diversified colours arranged in very singular forms, which Dr. Seebeck compares to the figures assumed by sand on vibrating pieces of glass, and discovered some time since by Professor Chladni; but which appear to have a still nearer resemblance to those which Comparetti has described, as produced by the admission of a beam of light into a dark room, through apertures of different forms; and we are much inclined to suspect that they depend on the

twofold transmission of the light to the eye, perhaps after repeated internal reflections, from the different points in the lateral surfaces of the substances employed. The effects were most conveniently observed in cubes of $1\frac{3}{4}$ inch, and better in white than in yellowish glass: in cubes of an inch only, they were indistinct: nor were they produced by fluor spar, rock salt, or by any kind of liquids: they were modified, and sometimes inverted, by the interposition of a plate of mica: and ice acted in a similar manner, in depolarizing the light transmitted through it. We find in these researches a full confirmation of the experiments which Mr. Malus had made some time before his death, to show, that the polarized light, which falls on a transparent medium at such an angle, as not to be reflected, is transmitted, with no material diminution of its intensity. Dr. Seebeck's language is a little enveloped in the mysticism of the school to which, by some singular caprice of fancy, he has thought proper to attach himself: but we cannot hesitate to believe, that as he continues his examination of the phenomena of nature he will by degrees be persuaded of the futility of the objections, which Mr. von Goethe has advanced against the Newtonian doctrine of the composition of white light, and of the inaccuracy of the assertions on which some of those objections are grounded.*

While the optical philosophers of France and Germany have been engaged in these researches, Dr. Brewster has been very laudably employed, in this country, in experimental investigations relating to the same interesting department of physical science. He has found that the agate cut by a plane perpendicular to its laminæ, transmits one only of the polarised portions of light; that the polarity of light may be destroyed by transmitting it in a certain direction through almost all mineral substances, and through horn, tortoise-shell, and gum-arabic: while in certain other directions its properties remain unaltered, whence he has distinguished, in these substances, different depolarizing and neutral axes; and that the light reflected from the oxydated surface of polished steel is so modified, as to

* There is an Article by Dr. Young in the Quarterly Review for the preceding January, Vol. X., p. 427, on Göthe's Treatise *Zur Farbenlehre*. As it contains very few original observations, it has not been reprinted.—*Note by the Editor.*

prove, in his opinion, that the oxyd is a thin transparent substance. His observations on the colours, sometimes exhibited by crystals of Iceland spar, seem to be identical with those of Martin and Malus.

Dr. Brewster has very ingeniously exercised his inventive powers in the contrivance of a variety of micrometers, goniometers, microscopes, and telescopes, several of which may very possibly be found useful in particular circumstances, although to others there appear to us to be many material objections: but, without referring to the test of experience, it would be of little utility for us to discuss their particular merits. Some detached remarks, however, we shall take the liberty of submitting to our readers, on passages of the work which appear to require correction. The advantage which Dr. Brewster attributes to the use of a transparent fibre for a micrometer, (p. 71,) is merely imaginary; since, although it is true that the central rays "suffer no inflection," this circumstance affords us no assistance whatever in judging when the rays are actually "central;" and the light transmitted by such a fibre, whenever the luminous object is in its neighbourhood, could only create confusion. In speaking of a telescope for the measurement of angular positions, Dr. Brewster observes that "the line, which joins any two stars, forms every possible angle with the horizon in the course of 23 hours and 56 minutes;" (p. 128;) but this is obviously a mistake; for at the poles of the earth the angle would not vary; and in other latitudes only within certain limits. The table of the variation of the focal length of a telescope (p. 218) is wholly erroneous, from the employment of linear feet and square inches in different parts of the same formula. Dr. Brewster has misunderstood Professor Robison and Mr. Wilson, where they observe that the focal length of an achromatic telescope must be lengthened, when it is directed to a star towards which the earth is moving (p. 221): it was not from the different distances of the stars, but from the difference of the relative velocities of light, that they argued, according to the general opinions respecting light, the necessity of the occurrence of such a minute variation. In p. 424, 5, the magnifying power is miscalculated, and we must read 4.9 for 5.6.

The most useful part of the whole work appears to be the series of experiments on the refractive powers of fluid and soft substances, performed by interposing them between the object glass of a microscope, and a plane glass nearly in contact with it, and then measuring the joint focal length of the combination. The comparative distances, thus obtained, are exhibited in several extensive tables: but we cannot help feeling some surprise, that the author has not attempted to deduce, from any one of his numbers, the direct refractive power of the substance concerned, as he certainly would have done if he had been aware how easily it might have been accomplished, after a preparatory investigation, dependent on the common laws of dioptrics. From such an investigation we have obtained formulæ for each of the two series of experiments; for the first (pp. 258, 268, 270,) f being the focal length expressed by the number in the table, and r the index of refraction, $r = 1.887 - \frac{1}{.8f}$; and for the second, (p. 264) $r = 2.31 - \frac{1.31}{f}$. Thus we obtain for phosphorus 2.125, sulfur 2.008, aloes 1.643, balsam of Tolu 1.636, oil of cassia 1.625, guaiacum 1.609, and pitch 1.589. Dr. Wollaston's Table gives for phosphorus 1.579, and for pitch 1.53; and there can be no doubt that the accidental presence of some phosphoric acid, and some oil of turpentine, on the surfaces of these substances occasioned an error, in these instances, in Dr. Wollaston's determinations, however excellent his method may be in other cases; for we cannot agree with Dr. Brewster, in thinking that the acknowledged exhibition of the index appropriate to the extreme red ray is an objection to the method. It is remarkable, as our author has justly observed, that the assignment of so high a refractive density to phosphorus restores the inference of Newton, respecting the relation between refractive powers and inflammability, to its original universality and importance.

Dr. Brewster's mode of ascertaining the refractive powers of solids, by immersing them in a mixture of fluids of equal refractive density, is perfectly unobjectionable; and he observes that it is easy to discover, in this manner, the internal flaws and other irregularities of gems, without the labour of polishing any part

of their surface. He does not, however, appear to have followed this method in determining the indices of refraction which are contained in his table, (p. 283,) having employed for this purpose "the same prisms in which the dispersion was corrected," and probably in the same manner: hence from an erroneous mode of computation his numbers are almost uniformly too large: thus we have phosphorus 2.224, sulfur 2.115, carbonate of lime 1 665 and 1.519, oil of cassia 1.641, and guaiacum 1.619, all of which exceed the more accurate determinations which we have already mentioned. In the same manner we find for diamond 2.487 to 2.470, instead of 2.439, the density assigned by Newton; and it is probable that the chromate of lead and realgar, both of which Dr. Brewster finds more dense than the diamond, are also rated somewhat too high at 2.974..2.503, and .2.549: the former appears to have a double refraction more distinct than any other known substance.

For a similar reason we can place no dependence whatever on the table of dispersive powers, which is calculated, according to a coarse approximation, wholly inapplicable to the circumstances of the experiments. The mode of inclining a prism of a greater density, until it caused the image of a right line, viewed through it and in conjunction with a prism of smaller density, to be colourless, would be a very good one, provided that the apparatus were so arranged, that the rays should be perpendicular to the common surface of the prisms; but even then Dr. Brewster's mode of calculation would be only applicable to prisms with very small refracting angles. In the only experiment which is related with precision, (p. 306,) the result implies an impossibility: for if we trace a ray of light through its intricate progress from the water to the glass, the angle of incidence upon the last surface will come out 41° 5′, while the utmost obliquity, at which it could have been transmitted, is 38° 14′, consequently the index of refraction assigned to the prism, 1.616, must be extremely erroneous if the angular measurements were correct. And since various errors of this kind may have affected the different results in different degrees, we cannot depend on the tables, even for the order of the differen dispersive powers.

Dr. Brewster appears, however, to have been more successful in confirming and extending the observations of Dr. Blair on the different proportions in which the prismatic spectrum is divided, according to the diversity of the substances which afford it. He has shown very clearly, both from theory and by experiment, that the violet rays must be proportionally more expanded by a prism with a large angle than by a smaller one of the same substance; while he has found, on the other hand, that a smaller prism of a more dispersive substance almost always expands the violet rays more than a large prism of a less dispersive substance; and that when two such prisms are combined, they exhibit a green fringe in the usual place of the red, and a "wine-coloured" fringe in that of the violet. The substances most expansive of the violet are oil of cassia and sulfur; the least expansive, sulfuric acid and water, although water has not quite so low a dispersive power as fluor spar. It seems to follow from Dr. Brewster's estimate, that the proportions of 2 red, 3 green, 4 blue, and 3 violet, which are nearly those of Dr. Wollaston's determination, are changed when sulfuric acid is employed, at least as much as to 4 red, 3 green, 3 blue, and 2 violet; but we feel great difficulty in believing that so great a variation as this could have escaped the notice of any attentive observer. We have no doubt, however, that if Dr. Brewster continues to pursue his ingenious investigations, he will by degrees acquire a habit of introducing greater accuracy into his measurements, and, what is of still more importance, more mathematical neatness into his calculations; and, with these improvements, we doubt not that his future labours may be productive of material benefit to those departments of physical science which have engaged his attention.

No. XV.

CHROMATICS.

From the Supplement to the Encyclopædia Britannica.

WRITTEN IN THE YEAR 1817.

THE gradual progress of scientific investigation has continued to add, from year to year, a multitude of new discoveries to our knowledge of experimental and physical optics: and no department of this subject has received additions so diversified and so important, as those which relate to the phenomena of colours, which have been displayed with a thousand brilliant and unexpected transformations, under circumstances that, in former times, could never have been suspected of exhibiting anything resembling them. The successive experiments and calculations of Dr. Thomas Young (1801), Dr. Wollaston (1802), Mr. Malus (1810), Mr. Arago, Mr. Biot, Dr. Brewster, Dr. Seebeck, and Mr. Fresnel, have all contributed very essentially to the extension and illustration of this interesting branch of science. But notwithstanding all that has hitherto been done, it appears to be utterly impracticable, in the present state of our knowledge, to obtain a satisfactory explanation of all the phenomena of optics, considered as mechanical operations, upon any hypothesis respecting the nature of light that has hitherto been advanced: it will therefore be desirable to consider the facts which have been discovered, with as little reference as possible to any general theory; at the same time, it will be absolutely necessary, as a temporary expedient, to borrow from the undulatory system Dr. Young's law of the interference of light, as affording the only practicable mode of connecting an immense variety of facts with each other, and of enabling the memory to retain them; and this adoption will be the more unexceptionable, as many of the most

strenuous advocates for the projectile theory have been disposed, especially since the experiments of Mr. Arago and Mr. Fresnel, to admit the truth of the results of all the calculations, in which this law has been employed. The details of its application to particular cases, together with an examination of the phenomena of polarisation and of oblique refraction, will occupy the principal part of this article; but it will also be necessary to premise an account of the few cases of the exhibition of colours which appear to be independent of its operation.

Section I.—*Of the Separation of Colours by Refraction.*

The separation of white light into different colours, as its component parts, by refraction, though firmly established as an optical fact by Newton, had been in general somewhat negligently examined as to its details, until Dr. Wollaston pointed out the inaccuracy of the common subdivision of the colours of the prismatic spectrum into seven different species. There is little reason to doubt, that white light consists of an infinite number of rays, varying gradually among each other, without any marked distinctions, and continued on the one hand into the dark chemical rays, and on the other into the rays of invisible heat; and that all these varieties are separable from each other by refraction, and preserve always a distinct and constant refrangibility. The species of homogeneous light, however, distinguishable from each other by the eye, are only five; red, yellow, green, blue, and violet; which are uniform in their appearance, and well defined in their limits, whenever a perfect spectrum is correctly exhibited; whether obtained by interposing a prism, between the eye and a small or rather narrow bright object, or between a lens and the image of such an object formed in its focus: while, in the common method of admitting a beam of the sun's light through a prism, without either employing a lens, or previously limiting the angular extent of the beam, it is obvious that there must be a double source of the mixture of colours; and hence has arisen the Newtonian division of the spectrum into seven parts, which were somewhat fancifully compared, with respect to their extent, to the intervals of the minor diatonic scale in music; although it has been

shown by Dr. Blair, and still more fully by Dr. Brewster, that their proportions are liable to very great variations according to the nature of the refracting substances employed.

Dr. Brewster has remarked, that as, according to the fundamental law of refraction, a prism with a large angle must occasion a dispersion of the several colours somewhat greater than two smaller prisms of the same substance, having together an equal mean refractive power; so also the dispersion of the most refrangible or violet rays amongst themselves will be always somewhat greater in a prism with a larger angle, than in two smaller prisms having an equal mean dispersive power; hence the green and blue will be less removed from the red towards the violet by the single prism, the refraction of the green remaining in defect when compared with the mean of the whole; so that if the two prisms be employed to correct the mean dispersion of the single one, and the extreme rays of the spectrum be brought to a perfect coincidence, the refraction of the green by these prisms being comparatively in excess, the green rays will be found on the side towards which their refraction tends to carry them; and the two extreme portions of red and violet will be left together, forming a crimson, on the side towards which the refraction of the larger prism is directed. It is obvious also that if, instead of the two smaller prisms, a single one of an equal angle, but of twice the dispersive power, were substituted, the joint effect would be nearly the same: Dr. Brewster has, however, observed, that in almost all such combinations of different substances, the green is on the side towards which the refraction of the larger prism is directed; so that the original proportion of the space occupied by the different rays in the spectrum must be different for different substances. Dr. Brewster has found that the violet is the most dispersed by oil of cassia and by sulphur, and least by sulphuric acid and by water: the distribution afforded by these substances appearing to vary from 2 parts of red, 3 green, 4 blue, and 3 violet, to about 4 red, 3 green, 3 blue, and 2 violet; while the yellow is always confined to a narrow line.

The immediate effects of the combinations of the primitive colours on the sense of sight afford an illustration of some of the

physiological characters of sensation in general. It is well known that a mixture of red and green light produces a simple sensation, perfectly identical with that which belongs to the minute portion of yellow light originally found in the spectrum: and that a mixture of green and violet makes a perfect blue. The blue colour of the flame of spirit of wine, for example, is derived entirely from a mixture of green and violet rays; while the blue light of the lower part of the flame of a candle is shown by the prism to consist of five different portions, belonging to different parts of the spectrum, nearly resembling those which would be distinguished, if we looked through a prism at a small portion of a transparent plate, of a certain minute thickness. It is obvious, therefore, that the eye has no immediate power of analysing such light; and if we seek for the simplest arrangement, which would enable it to receive and discriminate the impressions of the different parts of the spectrum, we may suppose three distinct sensations only to be excited by the rays of the three principal pure colours, falling on any given point of the retina, the red, the green, and the violet; while the rays occupying the intermediate spaces are capable of producing mixed sensations, the yellow those which belong to the red and green, and the blue those which belong to the green and violet; the mixed excitement producing in this case, as well as in that of mixed light, a simple idea only: although it must be observed, that no homogeneous light can extend its action so far as to excite at once the sensations of the fibres belonging to the red and the violet: so that every crimson must necessarily be a compound colour. A mixture of red and blue light exhibits an effect which appears unintelligible, upon the supposition that a compound light ought to produce a colour intermediate between those of its constituent parts; but this difficulty will vanish, if we assume that the blue of the spectrum contains a greater proportion of violet than of green; so that the green is neutralised into a white by a mixture with the red and part of the violet, and the remaining violet gives its character to the whole, either alone, or with a mixture of red, according to the proportions employed.

When we look through a prism at a luminous object of con-

siderable extent, surrounded by a dark space, the spectra belonging to the several parts of the object are mixed with each other, so as to produce a light perfectly white, except towards the ends of the object, where the extreme parts project beyond each other. At the red end of the spectrum, the whole of the red belonging to the extreme point retains its place unaltered, and the green and blue become a greenish yellow, nearly uniform in its appearance, throughout the space which belongs to them, while the place of the violet is scarcely distinguishable from the neighbouring white light; but at the opposite end, the violet retains its place and appearance, and the remainder of the length of the spectrum becomes of a green, inclining more or less to blue, and continuing to be very distinctly visibly throughout the extent of the simple spectrum, the place of the red included; so that the illuminating power of the red end of the spectrum must be incomparably greater than that of the violet end: as may also be inferred by a direct comparison of the distinctness of objects viewed in these different lights. The portion of light totally reflected at the internal surface of a dense medium, on account of the obliquity of its incidence, is bounded by a fringe or bow resembling the red end of the luminous object viewed through a prism; and the transmitted portion is bounded by the violet and blue fringe: but it requires some caution, in observing these colours, to avoid the optical deception, which causes the neighbouring space to appear of the complementary colour, especially when the eye is turned towards it immediately after having received the impression of the colours actually exhibited.

Section II.—*Of the Colours of Halos and Parhelia.*

The immediate effect of the different refrangibility of light, in the production of colours, is sometimes spontaneously exhibited, in the atmospherical phenomena of halos and parhelia, or paraselenes, attending the sun or moon; the edge nearest to the luminary being generally reddish, and the remoter parts more or less green and blue, although without any well marked separation of the different tints. These appearances have been

long ago referred by Mariotte to the refraction of the prismatic crystals of snow, floating in the atmosphere, and descending through it, in all possible positions, but more especially in a vertical or horizontal direction, on account of the effect of gravity, combined with that of the resistance of the air; and sometimes, perhaps, from their connexion with other crystals, making angles of 60° with either of these positions. This theory, however simple and satisfactory, had been very unaccountably neglected for more than a century, and even superseded by the awkward and unsupported conjectures of Huygens, respecting the existence of spherical or cylindrical particles of hail, including opaque nodules, related to them in a certain constant ratio; or by the equally inadmissible calculation of Newton, which assigns a partial maximum to the density of the light simply refracted through a spherical drop of water, when the deviation is about 26°; and it is only a few years since, that the doctrine of Mariotte was revived and extended by Dr. Young* and approved by Mr. Cavendish and Mr. Arago.

In some of the highest northern latitudes, these appearances of halos and parhelia are almost constant; and in warmer countries they are confined to the light clouds which occupy the higher and colder regions of the atmosphere. The halos are broad circles, with their interior margin tolerably well defined, and about the distance of 22 and 46 degrees from the sun or moon, but less distinctly terminated externally. Now the angle of 22° exactly corresponds to the deviation produced by a prism of ice, with a refracting angle of 60°, when it becomes a minimum from the equality of the angles of incidence and emergence; and in other positions of the prism, the deviation increases very slowly till it becomes a few degrees greater: hence the breadth of the circles of each colour being considerable, the colours must fall principally on each other, and become very indistinctly separated. The external circle may be referred to the effect of two such refractions in succession: Mr. Cavendish seems to have thought the angle somewhat too great to be derived from this source; and he suggested that it might depend on a single refraction by the rectangular terminations

* Lectures, vol. i., p. 442.

of the crystals: but it does not appear that such terminations are very commonly observable; and it may easily be shown, that the greatest intensity of the light of a halo, formed by two refractions, must be at more than twice the distance of the edge of the inner halo, derived from one only.

These halos are commonly accompanied by a white horizontal circle passing through the sun, derived from the reflection of the vertical faces of the crystals, which are scattered equally throughout all possible azimuths. There are also generally coloured parhelia on each side, depending on the refraction of these vertical prisms; they are commonly a little without the halos, because the deviation of the light passing obliquely through these crystals is somewhat greater than that of the light transmitted by the crystals which have their axes perpendicular to the plane of incidence and refraction. For a similar reason, the light passing through the crystals situated horizontally, in various azimuths, is variously modified, so as to produce the appearance of inverted arches, touching the halos at their highest points, and sometimes expanding in the form of a pair of wings, with a point of contrary flexure on each side.

The anthelia seem to be referable to two refractions and an intermediate reflection, within the same crystal, causing a deviation of about $120 + 22 = 142°$; and sometimes with two intermediate reflections, producing an angle of $60 + 22 = 82°$ only. It is not, however, very easy to assign a reason for the appearance of an anthelion exactly opposite to the sun, which is said to have been sometimes seen in the horizontal circle: but it has been delineated with the accompaniment of an oblique cross, and of other unusual appearances, which must have been derived from some extraordinary forms of the compound crystals of snow, existing, at the time of the observation, in the atmosphere.

Section III.—*Of the Colours of the Rainbow.*

The general nature of the primary rainbow was cursorily explained by De Dominis; but Descartes first applied the true law of refraction, which has lately been discovered, to the determination of the angular magnitude both of this and of the

secondary rainbow; although no sufficient reason could be assigned for the appearance of colours in either of them, until Newton ascertained the different refrangibilities of the different kinds of rays: but as soon as this discovery was established, the method of fluxions at once enabled him to determine precisely the limit, at which the broad expanse of light, belonging to each colour, must necessarily terminate in an edge of greater brilliancy; the bright edges of the different colours projecting gradually beyond each other, so as to form a spectrum somewhat mixed, but still approaching to the common appearance of a spectrum obtained by the refraction of a prism: and in fact, the angular distances of the exterior termination of the primary rainbow and of the interior of the secondary from the sun are found to agree very accurately with the calculation of the extreme deviations of the red rays reflected once and twice respectively within the spherical drops of rain; although the whole breadth of the coloured appearances is liable to variations dependent on the magnitude of the drops, and belonging to the phenomena of supernumerary rainbows, to be described hereafter.

The light reflected from very small portions of water appears to be incapable of producing a regular rainbow; thus we scarcely ever see a rainbow in a cloud, unless it has united its drops, so that they begin to descend in the form of rain. Dr. Smith has observed this circumstance, and has attributed it to a tendency of the bright edge of the expanse of light to lose its intensity, by being gradually dissipated into the neighbouring dark space: a tendency which he would probably have been much at a loss to explain from any of the received doctrines of optics, but which bears some analogy to the effects more commonly observed in beams of light admitted into dark spaces, and sometimes designated by the term diffraction.

SECTION IV.—*Of Periodical Colours in general.*

By far the greatest part of the phenomena of colours, except their separation by simple refraction, are referable to the description of periodical or recurrent colours: being characterised by an alternation, which is generally repeated, where the observation is sufficiently extensive, several times in succession,

while the circumstances, on which they depend, are varied uniformly and by slow degrees. The number of these alternations, when light perfectly homogeneous is employed, appears to be continued without any discoverable limit, although it is always smaller, for any given change of circumstances, when the least refrangible or red light is employed, than when the observation is made on the most refrangible or violet; so that mixed or white light always produces a combination of alternations arranged according to a series of different intervals, which are at first more or less distinct, but by degrees are so mixed with each other, as again to be lost in the general effect of white light. In all these cases, the appearances may be reduced to calculation by means of the general law of the interference of two portions of light, with its appropriate modifications and corrections.

A.—*The law is, that when two equal portions of light, in circumstances exactly similar, have been separated and coincide again, in nearly the same direction, they will either co-operate, or destroy each other, accordingly as the difference of the times, occupied in their separate paths, is an even or an odd multiple of a certain half interval, which is different for the different colours, but constant for the same kind of light.*

B.—*In the application of this law to different mediums, the velocity must be supposed to be inversely as the refractive density.*

C.—*In reflections at the surface of a rarer medium, and of some metals, in all very oblique reflections, in diffractions,* and in some extraordinary refractions, a half interval appears to be lost.*

D.—*It is said that, according to some late observations of Mr. Arago, two portions of light, polarised in transverse directions, do not interfere with each other.*

E.—*The principal intervals in air are for the*

Extreme Red	.0000266	$= \frac{1}{37640}$
Yellow	.0000235	$= \frac{1}{42550}$
Green	.0000211	$= \frac{1}{47400}$
Blue	.0000189	$= \frac{1}{52910}$
Extreme Violet	.0000167	$= \frac{1}{59750}$
Mean, or White	.0000225	$= \frac{1}{44440}$ inch.

* See No. XVII., p. 392, note.

Section V.—*Of the Colours of Thin Plates.*

The colours exhibited by very thin plates of transparent or semitransparent substances have been well known to optical philosophers, from the time that they were first noticed by Boyle, and more particularly examined by Hooke and Newton. They may be readily observed, by pressing together any two clean pieces of common plate-glass, which have always sufficient convexities and concavities to exhibit them, touching each other in some points, and leaving elsewhere a thin plate of air between them; or still more conveniently, by selecting from the planoconvex lenses, kept by the opticians, such as have their flatter sides very slightly convex, and are consequently calculated to throw the spaces of equal thickness, and the colours dependent on them, into the form of rings. The colours are most distinct when they are formed in the light reflected from the two surfaces in contact, especially when care is taken to exclude the foreign light, reflected by the surfaces not concerned in their production: and in this case they begin from a central dark spot, immediately surrounded by a bright light, and then by rings more distinctly coloured; while the colours, exhibited in light transmitted through the glasses, begin from a bright spot in the centre, surrounded by a dark ring; being always exactly complementary to the colours seen by reflection; to which they are also, as Mr. Arago has demonstrated, either exactly or very nearly equal in intensity; although they have generally been supposed to be much less vivid, on account of the diminution of their effect on the eye by their mixture with the whole of the beam of light which affords them. But if we employ, for the observation, two flattish pieces of glass, held in such a position as to transmit the light received from one part, and to reflect an image of another part, of an object equally illuminated throughout its extent, the two series of colours will destroy each other, and the whole appearance of rings will vanish: when, on the contrary, the illumination of the object varies materially, the rings will reappear in one or the other of their forms, according to the different intensities of the lights

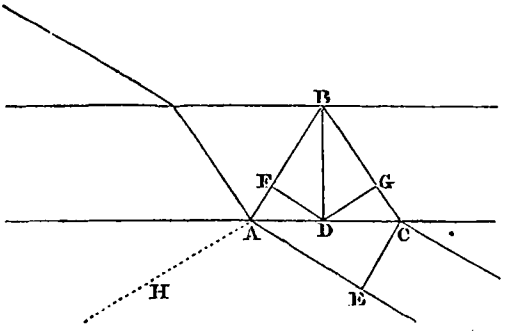
received from its different parts: so that, as Mr. Arago has ingeniously suggested, this test might be employed to answer the purpose of a photometer, for ascertaining the equality of the lights of two distant objects.

If any thin plate, affording colours, be inclined to the direction of the light passing through it, the appearance of the colours will be changed either precisely or very nearly in the same manner as if the thickness were reduced, in the ratio of the radius to the cosine of the inclination within the plate; at least, if this proportion is not perfectly accurate, the deviations from it, in the experiments of Newton, are manifestly within the limits of the unavoidable errors of observation.

We are indebted to Mr. Arago for the important fact, that the colours, observed in transmitted light, are distinguished by a polarisation opposite or transverse to that which is appropriate to transmitted light in general, and possessing the ordinary character of the polarisation produced by partial reflection. It is in light thus reflected that we must seek for one of the two portions which are to be combined according to the laws of interference, in the case of the colours seen by transmission, and for both in the case of reflection. The light transmitted simply through the plate will be followed by a portion which has been reflected back from the second surface to the first, and forwards again from the first to the second, and the difference of the times, occupied in these different paths, will obviously be proportional to the thickness of the plate, and also, according to the modification (B) of the law, to its refractive density: so that the number of alternations of any given colour, between the central spot of the rings and any given point, will be as the thickness of the plate at that point; and the numbers for different colours will be inversely as the magnitudes of the appropriate intervals; the plate appearing light, when illuminated by a homogeneous colour only, where the thickness corresponds to any exact multiple of the interval, and dark at the intermediate points; and this proportion is found to agree perfectly with experiment. The two reflections within the plate, being always of the same kind, will either not require any correction on account of their nature (C), or will together add a whole inter-

val to the length of the path, an alteration which makes no change in the appearances.

When the incidence is oblique, the actual length of the two passages of the reflected ray across the plate, AB, BC, is as twice the secant of the angle of refraction ABD, and its advance upon the surface, AC, as twice the tangent: and this advance, reduced to the direction of the transmitted ray AE without the plate, must be subtracted from the retardation within the plate; the reduction being in the proportion of the radius to the sine of the angle of incidence ACE, for which if we substitute that of the radius to the sine of the angle of refraction ADF or CDG, we shall have the deduction required to be made from



the length of the path within the plate, since the velocities vary directly as these sines; and by this deduction the secants, AB, BC, will be reduced to the cosines BF, BG: so that the true retardation will always be proportional to the cosine of refraction.

The same demonstration is applicable to the difference of the paths of the two portions of light reflected once only from the upper and lower surfaces of the plates respectively; supposing A, the point of emergence of the transmitted ray, to become the point of incidence of a new reflected ray HA. Hence it might be expected that all the phenomena of colours should be the same as in the case of transmitted light; and this really appears to happen when the observation is made on a plate of air contained between a transparent substance and a polished surface of gold or silver; or on a plate of a refractive density intermediate between the densities of the neighbouring substances,

as in the instance of a thin coat of smoke or of an oxide, adhering to any polished metallic surface, which is at first of a yellowish white, and, as it becomes thicker, changes to a yellow and an orange colour; but in more common cases there is a loss of half an interval in one of the two reflections only, so that the thicknesses affording a perfect coincidence, for any species of colour, are always intermediate between the thicknesses affording the same colour by transmission; and hence the tints of the two series of rings are always complementary to each other, the series seen by reflection always beginning from a dark central spot, when they are exhibited by any detached transparent substance, as a soap bubble, a thin film of glass, or of talc, or by a plate of air contained between two plates of glass, or between a plate of glass and a piece of polished steel.

There is a peculiarity in the surface of silver and gold, and perhaps of some other metals, that, besides the regular reflection at an angle equal to that of incidence, a considerable quantity of light is dispersed irregularly; and this light, as Mr. Arago has observed, is polarised in a direction transverse to that of the usual polarisation by reflection; there is also in the irregular reflection no loss of a half interval; so that it exhibits, with a piece of glass, a series of rings resembling those which are produced by polished steel, except that their dimensions are not varied exactly in the same proportion by the obliquity of the incidence, because the light which forms them is not required to pass towards the metal in an angle exactly equal to that which it makes upon its return after reflection; and there will probably be considerable irregularities in the interval of retardation, according to the mode of performing the experiment; although in general the irregular dispersion or diffraction from the glass is too weak to afford colours easily observable, when the position of the plate differs considerably from that in which the light is regularly reflected. If a portion of polarised light is incapable of interfering with another portion polarised in a transverse direction, these rings ought to disappear when the angle of incidence on the plate of glass is about 55°, since in this case the light reflected by it is completely polarised in the plane of incidence; and this disappearance seems actually to

have been observed in some of Mr. Arago's experiments, though in others, where the metallic surface was less highly polished, the polarisation of the dispersed light may have been less complete, and the rings may still have been visible at this angle. (*Mémoires d'Arçueil*, Vol. III. p. 354, 359.)

Section VI.—*Of the Colours of Double Plates.*

When light is transmitted in succession through two plates, differing but little in thickness, they exhibit an appearance of colour similar to that which would be produced by a single plate equal in thickness to their difference; and this appearance is wholly independent of the distance of the plates from each other. It was first noticed by Mr. Nicholson, in the glasses employed for the sights of sextants, and is attributed by Dr. Young to "the rays twice reflected within the first glass only, interfering with the rays twice reflected in the second only:" in some circumstances, however, the light returning from the second glass to the first, and again reflected by it, may co-operate in the effect; the interval of retardation being the same in both cases. Mr. Knox has more lately described some very striking appearances of colours obtained in this way, by the combination of two pairs of lenses, each exhibiting their appropriate rings when viewed separately, and affording together a third series of rings of larger dimensions, when the two former are unequal in magnitude, and of straight bands when they are equal. It is in fact easily demonstrable that in order that the thicknesses of the plates of air, contained between two unequal pairs of lenses, may be equal, the distances from the centres of contact must be in a constant proportion; and it is well known that all the points, from which the lines drawn to two given points are in a constant proportion, will be found in the circumference of a circle, the diameter of which is a third proportional to the difference and sum of the segments of the given distance of the points; so that the colours depending on this difference, instead of beginning, as usual, from a white central spot, will begin from a white ring, and will be arranged in concentric rings on each side of it, precisely in the same order as when

they form concentric rings round an actual point of contact: and when the curvatures of the two pairs of lenses are equal, the diameter of the circle becoming infinite, it will obviously be converted into a right line.

Dr. Brewster has observed a series of similar phenomena, produced by two plates of equal thickness, but forming a small angle with each other, so as to be differently inclined to the light passing through them. The effect of the inclination being to reduce the virtual thickness of the plate in the ratio of the cosine, and the difference of the cosines of equi-different arcs being simply as the sine of their half sum, it is evident that the colours must correspond to a thickness which varies nearly as the sine of the angle of incidence, considered with regard to a plane bisecting the angle formed by the plates: and this result agrees correctly with Dr. Brewster's experiments.

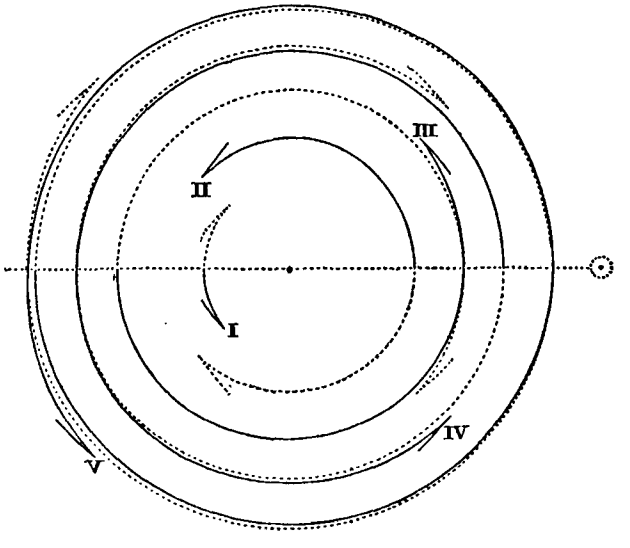
Section VII.—*Of the Colours of Supernumerary Rainbows and Glories.*

Within the common primary rainbow, and without the secondary, we sometimes observe a partial repetition of colours, more or less distinctly marked, and extending occasionally to several alternations; the repetitions occupying somewhat narrower spaces as they are more remote from the ordinary bows. These appearances seem to have been first described by Mariotte; they have been since noticed by Langwith, Daval, and Dicquemare: and the term supernumerary rainbows has been very properly applied to them. The coloured circles, called glories, may generally be seen surrounding the shadows of our heads, when we have an opportunity of standing on a high hill, and observing them in a cloud below us: they are also sometimes accompanied by a large white circle, which, in an observation of Ulloa, was 67° in diameter; and such a circle may frequently be distinguished when the sun shines on a mass of vapour rising from a warm bath, of nearly the same dimensions, or sometimes a little smaller. The whole of these phenomena may be explained from the interference of some of the portions of light regularly reflected within the minute drops of water

with other portions, incident at a different angle, but, after an equal number of reflections, coinciding ultimately with them in direction; supposing only the clouds in question to afford a number of these drops varying but little from each other in diameter. We find by the well known mode of calculating the greatest deviation that each order of reflections exhibits a zone from 30° to 10° in breadth, through which a double light is diffused by each drop; and, besides this, when there have been more than three reflections, the portions belonging to the opposite sides cross each other in one or more points, and surround the drop; or rather the observer, if we consider the effect of the refraction of a multitude of drops situated in all directions. Supposing the index of refraction for the extreme rays 1.336, and its logarithm .1258000, the results will be these —

After	Extreme Deviation.	Final Deviation.
1 reflection	41° 40′	13° 52′
2 „	51 41	69 12
3 „	40 39	27 44
4 „	45 2	55 20
5 „	50 0	41 36
6 „	34 14	41 28

We may obtain a more distinct idea of these duplicatures if we represent them in a diagram, showing the angular extent of



the diffusion of light derived from each order of reflections, and distinguishing by different kinds of lines the portions belonging to the opposite halves of the drops: and it will be obvious, from the inspection of this figure, that the appearances in question have only been observed within some of the duplicatures of the orders to which they belong, between the angles of extreme and of final deviation. The tertiary and quaternary bows (III. IV.) are evidently too near the luminary to be visible: the quinary (V.) ought to be seen in the space between the primary and secondary, but it is probably much too faint to be visible under any circumstances. The duplicature belonging to the primary rainbow exhibits two portions, for which we may calculate the interval of retardation in parts of the radius of the drop, supposing the velocity to be that which is appropriate to the air, by taking twice the difference of the cosines of incidence on the drop, and multiplying twice that of the cosine of refraction by the index 1.336; the difference of these differences giving the interval for the two portions, of which the direction has been found to coincide by a previous calculation.

Distance from the Edge.	Angle of Reflection.	Difference of the Paths.	Distance from the Edge.	Angle of Reflection.	Difference of the Paths.
0°	40°. 2′	.0000	5°	45° 46 / 30 34	.0160
1	42 59 / 36 23	.0014	6	46 9 / 29 26	.0210
2	44 2 / 34 32	.0040	8	46 45 / 27 20	.0327
3	44 45 / 33 3	.0074	10	47 12 / 25 24	.0461
4	45 20 / 31 45	.0113	12	47 32 / 23 33	.0612

Hence it may be inferred that, supposing the extreme red to re-appear at the distance of 2° from the primitive external termination of the rainbow, the radius of the drop must be $\frac{.0000266}{.004}$ = .00665, or $\frac{1}{150}$ of an inch; the fourth alternation of the red being at the distance of 5°, where the interval is .016. The magnitude of the interval, at an equal distance from the edge, varies but little with the refractive density: thus, for

violet light, the index of refraction being probably about 1.346, and its logarithm .1290000, the greatest deviation will be found 40° 14′; and for a deviation 2° less, the angles of refraction must be 43° 30′ and 33° 47′, and the interval will be little different from .00400.

The supernumerary bands of the secondary bow, formed by the same drops, will be a little broader than these, since it appears, from a similar calculation, that the rays interfering with each other, at the distance of a degree from the edge, will exhibit an interval of .0011 of the radius only, instead of .0014.

The supernumerary colours of the third and fourth bows will be equally imperceptible with the bows themselves: but the portions of light, four times reflected, will cross each other in the point opposite to the sun, where their coincidence will be perfect, and at other neighbouring points will afford an interval nearly proportional to the distance from that point. We shall find that the intervals for different deviations, supposed to be measured in air, are these:—

Deviation.	Angle of Reflection.	Interval in parts of the Radius.
180°	24° 49′	.000
185	25 31	.096
175	24 7	
190	26 14	.195
170	23 25	

Hence, supposing the first bright or greenish ring to appear at the distance of 5° from the observer's head, the radius of the drops must be about $\frac{.0000225}{.096}$ = .000234, or $\frac{1}{4270}$ of an inch.

It might be questioned, whether the light, five times reflected, could retain sufficient force to produce any sensible effects by these interferences, but since it exhibits no appearance of colour between the primary and secondary rainbows, it must necessarily be extremely faint. The interval which it affords, by the comparison of its two portions, agrees sufficiently well with that which is derived from four reflections, to contribute in some measure to the production of an alternation of light and shade;

but the separate colours would be rather weakened than strengthened by the mixture: thus, at the deviation of 5°, the interval is found to become .076 instead of .096; and at 10°, .155 instead of .195: and this difference is too considerable to allow us to expect any material increase of brilliancy from the addition of the fifth reflection, however great its intensity might be.

Supposing now a cloud to consist of spherules of which the radius is .000234, we may inquire at what distance from the outer edge of the primary rainbow the first additional red of the supernumerary colours ought to be found: the interval being in parts of the radius $\frac{.0000266}{.000234} = .116$: and we may infer from the table, by taking the successive differences, that this distance will be about 18°; so that the semidiameter of this red ring will be $42 - 18 = 24°$: and the termination of the primitive band of red, supposing it to extend to one fourth of a complete interval only, will be where the difference is .029, or at $7\frac{1}{8}°$; but for the violet the quarter of the interval will be, in parts of the radius, $\frac{.0000042}{.000234} = .0183$, which answers to a distance from the edge of about $5\frac{1}{2}°$: and this distance, measured from the edge of the violet, which is somewhat less than 2° within that of the red, will extend nearly to the same point as the red space: so that we shall have a circle, about 70° in diameter, at the circumference of which all the colours will be united, and which will consequently be white. This magnitude agrees tolerably well with the direct observations of the phenomenon; and if we wish to make the agreement more complete, we have only to suppose the drops a little smaller, and the coloured glories, which they are capable of affording, a little larger. It has already been remarked, that the non-appearance of the ordinary rainbow, in this case, must be referred to the operation of something like diffraction; although it is obvious that its form, under such circumstances, would necessarily be somewhat modified by the diffusion of the colours through a greater space than that which they ordinarily occupy.

Section VIII.—*Of the Colours of Striated Substances.*

It was observed by Boyle, that small scratches of any kind, on the surfaces of polished substances, exhibited, when viewed in the sunshine, a variety of changeable colours; and the observation may easily be repeated with any piece of metal, not too highly polished, and placed in a strong but limited light. Dr. Young ascertained by experiment that the colours afforded by some regular lines, drawn on glass, always corresponded to an interval, varying as the sine of the angle of deviation from the position, in which an image of the luminous object was exhibited by the regular reflection of the surface; and it is easily shown that, if we suppose two portions of light to be reflected upon the opposite edges of the furrow, the difference of their paths must vary in that proportion. Dr. Young had conjectured that the colours of the integuments of some of the coleopterous insects might be derived from furrows of this nature; but the conjecture has not been verified by observation. Dr. Brewster has, however, very unexpectedly discovered that some similar inequalities are the cause of the colours exhibited by mother of pearl; and he has confirmed the observation by showing that impressions of the surface of this substance, taken in black wax, in a hard cement, or in fusible metal, will often exhibit a similar appearance. Where the form of the surface of the mother of pearl is the most regular, it reflects, in an oblique light, a white image of a luminous object, like that which any other polished substance affords; but on one side of this image only, and at some little distance from it, we may observe the first order of recurrent colours, beginning from violet, and occasioned in all probability by the reflections from one side only of an infinite number of parallel striæ, formed by the terminations of a minute lamellated structure, nearly, but not perfectly perpendicular to the general surface; one side only of each of the little furrows being situated in such a direction as to reflect an image of the luminous object to the eye, and at such a distance that the whole may constitute a regular series of equal intervals. By transmitted light this

substance generally appears of a red or a green colour, changing more or less according to the obliquity, and apparently belonging to some of the higher orders of recurrent colours.

Dr. Young has observed a series of these colours, produced by the parallel lines of some of Coventry's glass micrometers, drawn at the distance of $\frac{1}{500}$ of an inch from each other, in which the first bright space, or the confine between the green and the red, corresponded to the interval of $\frac{1}{4300}$ of an inch, or .0000232;* and this result agrees very accurately with the general theory, the interval for the yellow, derived from Newton's measurements, being .0000235; but in general these lines exhibit colours much more widely extended, each separate line consisting in reality of two or more scratches, at a minute distance from each other.

There is a remarkable peculiarity in the appearance both of these colours, and of those which are exhibited by substances naturally striated, as by mother of pearl, agate, and some other semi-transparent stones; they lose the mixed character of periodical colours, and resemble much more the ordinary prismatic spectrum, with intervals completely dark interposed. This circumstance may be satisfactorily deduced from the general law, if we consider that each interference depends not only on two portions separated by a simple interval, but also on a number of other neighbouring portions, separated by other intervals which are its multiples; so that unless the difference of the two paths agrees very exactly with the interval appropriate to each ray, the excess or defect being multiplied in the repetitions, the colour will disappear; consequently, each of the stripes which, in other cases, divide the space in which they appear almost equally between light and darkness, when homogeneous light is employed, becomes here a narrow line; and their succession affords a spectrum exhibiting very little mixture of the neighbouring colours with each other, and nearly resembling that which is afforded by the simple dispersion of the prism; except that, as in all other phenomena of periodical colours, the blue and violet portions are much more contracted than in the common spectrum.

* See p. 356 of this volume.

Section IX.—*Of the Colours of Mirrors and of thick Plates.*

In all the species of periodical colours which have been described, the two portions of light concerned have both been regularly reflected from different surfaces. The methodical division of the subject now leads us to the consideration of the colours exhibited in light separately reflected from the same surface. These may be denominated in general the colours of mirrors; and they will include, as a variety, those which are called by Newton the colours of thick plates.

The general character of these colours is, that they are observed in light reflected by small particles, or irregularly dissipated by a single surface, first in the passage of the beam of light towards the mirror, and then in its return: the difference of the length of their paths affording, as usual, the interval of retardation. Thus in Dr. Herschel's experiment of scattering a fine powder in a beam of light reflected perpendicularly by a concave mirror, and received on a screen in its return, it may easily be shown that the colours will be precisely such as would be exhibited by light transmitted through a thin plate of air, everywhere half as thick as the plate limited by two spherical surfaces in contact; the centre of the one surface being the particle of powder, and that of the other its image formed by the mirror. For in the direction of the principal ray, which is perpendicular to the mirror, the paths of the light will be of equal length, whether the dissipation takes place before or after the reflection; and in other parts, the whole length of the path of the light passing from any focal point to its conjugate focus being the same, according to the definition of a conjugate focus in the Huygenian theory, from whatever point of the mirror it may be reflected, the light first dissipated will have advanced, after its reflection, as far as the circumference of a circle, of which the conjugate focus is the centre, at the same instant that the portion coming directly from the powder, after a previous reflection, will reach the circumference of the circle of which the particle of powder is the centre; so that the distance be-

tween these two circles must be the difference of the paths of the two portions, and the colours the same as would be exhibited by a plate of air of half the thickness, since such a plate is twice traversed by the retarded light.

A similar appearance of colours had been obtained, by earlier experimenters, from the interposition of a screen of gauze, or of a semi-transparent substance, in the path of the beam falling on the mirror. But the colours of thick plates, observed by Newton, are modified by the nature of the transparent substance employed, and by the obliquity of the refracted light. The dissipation here takes place at the anterior surface of a concave mirror of glass, and the reflection at the posterior, which is coated with quicksilver: and if these two portions proceed, each with a slight divergence, from a perforation in a screen situated near the centre of curvature of the mirror, they will co-operate perfectly with each other in the circumference of a circle described on the screen, of which the diameter is the distance of the perforation from its image; since all the light passing, in any given section of the mirror, with the same obliquity, through the glass as the beam itself passes in the principal section, must be collected into a focal point situated in some part of this circle, and will arrive at this point at the same time, whatever its situation in the section may have been: the obliquity of the incident light being the same in every part of the section, because the point of divergence is at the same distance from the mirror as the centre of curvature. For the other parts of the dissipated light, passing with different obliquities, the interval will be determined by the difference between the lengths of the paths of the two portions of light arriving at the given point, the one by regular refraction, after being first dissipated and then reflected; the other by dissipation, after being first regularly refracted and reflected. And this interval agrees precisely with the law which Newton has deduced from his experiments; but the analogy which he infers from it, between these colours and those of thin plates, is in fact very far from amounting to identity: since, if they belonged to the ordinary colours of thin plates, there is no reason why the series should begin anew from a certain arbitrary

thickness, differing in every different experiment, which affords a white of the first order.

Section X.—*Of the Colours of deflected Light.*

We are next to examine the case of light only once reflected, and interfering with a portion of the same beam which has pursued its course without interruption: a case which would scarcely have required a separate consideration, but from the difficulty of including it in a general definition with any others; although it is comprehended in the Newtonian description of the colours of inflected light: but since the light is in this case turned away from the substance near which it passes, it may more properly be termed deflected, especially as the greater number of the appearances, mentioned by Newton, as depending on inflection, belong more properly to diffraction, and the term inflection might consequently be misunderstood as relating to them.

When a beam of light is received in a dark room, and suffered to fall upon the edges of two extremely sharp knives or razors, meeting each other in a very acute angle, the shadows of the knives, received on a screen at some distance, will be found to be bordered by several fringes of colours; and the angle will be bisected by a dark line. The distances from the shadows, at which these fringes appear, agree in general with the supposition of their depending on the interference of the light, reflected from the edges of the respective knives, with the uninterrupted light of the beam passing between them: but the coincidence of these portions ought to be perfect in the immediate neighbourhood of the point in which the shadows meet, and the two last bright fringes ought to unite there in an angle of light. This, however, does not happen on account of the modification of the general law (C), which makes it necessary to allow half an interval for the effect of a very oblique reflection: and for the same reason, the space immediately next to the shadow is always dark instead of being light. If the knives are at all blunt, the reflection from one to the other, where they meet, causes the bisecting dark line to disappear; but this source of

error may be avoided by causing one of them to advance a little before the plane of the other.

Mr. Fresnel has repeated these experiments with all possible care, and has ascertained that the points, in which the fringes of any one colour are found, at different distances from their origin, belong always to a hyperbola, as they ought to do according to the calculation founded on the general law of interference; a fact which had before been inferred from other measurements, but which had not been so distinctly proved by direct experiments. Newton himself, indeed, was so far from believing that these fringes are rectilinear, as Mr. Fresnel supposes, that he expressly mentions their curvature, and infers from it that they are not derived from "the same light" in all their parts; imagining, perhaps, that each fringe was of the nature of a caustic line, formed by reflection or refraction, in which the light is everywhere more condensed than in the collateral spaces, but which is by no means necessarily straight. Mr. Fresnel has also shown, that all the fringes are found exactly at such distances from the true shadow, as would be inferred from the supposition of the loss of half an interval by reflection; while some of the experiments of Newton appeared to indicate a deviation from this law. It has been asserted, that fringes of the same kind have been observed at the edges of a detached beam of light, reflected into a dark space by a narrow plane and polished surface; and in this case it would be difficult to point out in what manner the supposed oblique reflection could be produced, or how a diffraction of any kind could cause the light to be redoubled back upon itself: but the experiment does not appear to have been hitherto performed with sufficient attention to all possible sources of error.

Section XI.—*Of the Colours of diffracted Light; including those of Fibres, and of Coronæ.*

The light reflected from each of the knife edges, in experiments like those of Newton, not only produces colours by its interference with the light proceeding uninterruptedly between them, but also with another portion, diverging from the edge

of the opposite knife, and spreading into its shadow. This tendency of light to diffuse itself was first described by Grimaldi, under the appropriate name diffraction: but many of the phenomena, in which it is concerned, having been attributed by Newton to other causes, he appears almost to have overlooked its existence.

The general law of interference is very directly applicable to all phenomena of this kind: the fringes exhibited are broader in the same proportion as the distance between the edges is narrower; and they always depend on the difference of the distance from the edges as the interval of retardation. It is however necessary to suppose the same modification to take place in diffraction as in oblique reflection, half an interval being lost in both cases; since the light which deviates the least from a rectilinear direction, and which is derived from the near approach of the two paths to equality, is always white. But it is remarkable, that when the obliquity becomes a very little greater, the diffracted light seems to change its character in this respect; for the colours occupy the same spaces as would have belonged to them, if they had begun from a dark centre, one of the portions only having lost a half interval in comparison with the other: and of this circumstance no explanation has yet been attempted.

The diffraction producing these fringes may easily be detected within the eye itself, by holding any object near it, in such a position as to intercept nearly all the light of a candle except a narrow line at the edge; this line will then appear to be accompanied by other lines parallel to it, separated from it by a dark space, and becoming wider when the object is brought nearer to the eye. These fringes must be referred to the light diffracted on one side round the object, so as to be spread on the unenlightened part of the retina, and reflected on the other from the margin of the pupil: for if we employ an object narrower than the pupil, so as to observe them on both sides of it, their magnitude will be altered by any change in the aperture of the pupil, occasioned by admitting light to the opposite eye, or otherwise. In such cases as this, where one of the points of divergence is much nearer to the point of

interference than the other, the interval increases more rapidly than the distance from the primitive direction; and the first fringes are much broader than those which succeed them; the mode of their formation approaching to that of the fringes seen in deflected light, commonly called the exterior fringes of the shadow; while the interior fringes belong more immediately to the present subject, that of the colours of diffracted light.

When the distance of the points of divergence is more nearly equal, the one being collateral to the other, the breadth of the successive fringes is also more uniform. Such is the appearance of the colours exhibited by a number of equal fibres held between the eye and a distant luminous object: their origin being identical with those of the fringes produced in the shadows of the knives; except that the diffracted rays come from the remoter side of the fibres, and follow the reflected rays, instead of preceding them. These colours may easily be observed by looking at a candle through a lock of fine wool, and still more distinctly by substituting for the wool some of the seeds of the lycopodium, strewed on a piece of glass; and they become very large if we employ a few of the particles of the blood, or the dust of the lycoperdon, or puff ball. Dr. Young has made this appearance the foundation of a mode of measuring the fineness of wool, which he has recommended for agricultural purposes, though it seems hitherto to have been found much too delicate to be employed by "the hard hands of peasants," with any advantage. The instrument, which he has invented for this examination, is called the eriometer, and its scale is calculated to express, in semidiameters of a circle, formed round a central aperture in a card, or a plate of brass, and marked by minute perforations, the distance at which the lock of wool must be held, in order that the first bright ring of colours, or the limit of the green and the red surrounding it, may coincide with the circle of points: and the actual measure, expressed by a unit of this scale, is found to agree very nearly with the thirty thousandth of an inch. Thus the particles of water, which have been found capable of exhibiting a glory 5° from the shadow of the observer, being about $\frac{1}{1185}$ of an inch in diameter, they would correspond to number 14 of this

scale; and the cotangent of the angle subtended by the semi-diameter of the bright circle being 14, the angle itself will be about 4°; consequently, if we looked at the sun through such a cloud, he would appear to be surrounded by a bright circle of colours, 8° in diameter, green within, and red without, and attended by other colours, more or less distinctly marked, according to the degree of uniformity of the magnitude of the drops. These circles are called coronæ: their dimensions vary considerably: but they have seldom been observed quite so large as these drops would make them; and more commonly they seem to depend on drops about a thousandth of an inch in diameter; although it is not easy to ascertain the precise parts of the rings, from which the measures have been taken by different observers.

In the shadow of a larger substance, formed in a beam of light admitted into a dark room, these colours are still perceptible, beginning from a white line in the middle; but here both the portions, on which they depend, are diffracted into the shadow; and beyond its limits, they are lost in the stronger light that passes on each side of it. Their appearance is somewhat modified, when the shadow is formed by a body terminating in an angle; for the breadth of the fringes being inversely as the breadth of the object which forms them, it is obvious that this breadth must increase towards the point of the shadow, like the distance of the fringes formed in the shadows of Newton's knives: and the fringes seen within the angle must necessarily assume the character of hyperbolas: nor will this form be materially altered, when the angle becomes a right one, as in the crested fringes, noticed by Grimaldi; although the steps of the calculation, for determining their magnitude, are in this case a little more complicated.

We find, in an elegant experiment of Mr. Biot, on the fringes produced by diffraction, a singular confirmation of the truth of the theory, which derives these colours from the difference of the times occupied in the passage of the different portions of light to the point of interference: although this celebrated author does not seem to have been aware of the nature of the inference which may so naturally be drawn from it. He found

that the densities of the substances, from the margin of which the diffracted light originated, had no influence whatever on the appearances produced by them: but when they were formed in the light diffracted from substances placed at one end of a long tube, and observed on a piece of glass fixed at the other end, they became contracted, upon filling the tube with water, in the proportion of 4 to 3; as was to be expected from the diminished velocity which must be attributed, according to the modification of the general law (B), to the passage of the light through a denser medium.

Section XII.—*Of the Colours of Mixed Plates.*

The colours of mixed plates depend partly on diffraction, and partly either on reflection or on direct transmission: but their essential character consists in the different nature of the two mediums, through which the light passes after its separation.

When a minute quantity of moisture is interposed between two lenses, it readily divides itself into a great number of smaller portions, scarcely distinguishable by the eye: and the light transmitted through the lenses exhibits rings of colours much larger than those which are ordinarily observed, and depending on the interval afforded by the difference of the velocities in the different mediums, according to the inverse proportion of the refractive densities. If they are viewed in a direct and unconfined light, the rings belong to the series commonly seen by transmission, beginning from a light central spot: both portions passing in this case simply through the separate mediums, and arriving at the eye after some slight diffraction only, which affects both of them in an equal degree: but if a distant dark object is situated immediately behind the lenses, and they are illuminated by a light incident a little obliquely, their character is changed, and they resemble the colours commonly seen by reflection, one of the portions of light being necessarily reflected, as in the case of the colours of deflected light: so that, when the dark object is situated behind one half of the glasses only, we observe the halves of two sets

of rings, of opposite characters, exhibiting everywhere tints complementary to each other. The diameters of the rings vary according to the refractive density of the liquid employed, diminishing as that density increases, and becoming much larger when two liquids, incapable of mixing with each other, and differing but little in refractive density, as oil and water, are employed instead of air and a single liquid.

The magnitude of the interval may also depend on that of a minute transparent solid substance, immersed in a liquid, instead of being limited by the distance of the two lenses: thus the dust of the lycoperdon, mixed with water, gives it a purplish hue, when seen by indirect, and a greenish by direct light: and when salt is added to the water, or oil is substituted for it, the difference of the velocities being lessened, the colours exhibited rise in the series, as if the plate were made thinner.

Mr. Arago has very ingeniously applied the principle of the production of these colours, to the construction of an instrument, for measuring the refractive densities of different elastic fluids, and of air in different states of humidity; the fluids being contained in two contiguous tubes of a given length through which the two portions of light are made to pass, previously to their reunion, and to the formation of the bands of colours; and it may easily be conceived, that the delicacy of such a test must be great enough for every determination that can be required, either for the correction of astronomical observations, or for the illustration of the optical properties of chemical compounds.

Section XIII.—*Of the Laws of the Polarisation of Light.*

The colours first observed by Mr. Arago, in doubly refracting crystals, and since more particularly analysed by Mr. Biot, afford by far the most striking and interesting examples of the colours of mixed plates. In order to understand the laws of these phenomena, it is necessary to be previously acquainted with the affections of polarised light, which were first accurately investigated by Malus, and with the theory of extraordinary refraction, derived by Huyghens, with equal ele-

gance and precision, from his peculiar hypothesis respecting the nature of the transmission of light.

1. Mr. Malus discovered, that at a certain angle of incidence, the light partially reflected, by a transparent substance, receives a peculiar modification with respect to the plane of reflection, which is called *polarisation in that plane.*

2. Dr. Brewster observed, that the *angle of complete* polarisation is such, that the mean direction of the transmitted light is perpendicular to that of the reflected portion; the tangent of the angle of incidence being equal to the index of the refractive density of the medium.

3. A ray of polarised light is again subdivided, in the usual proportion, by a second refraction in the plane of polarisation: but when it is refracted in a *plane perpendicular* to the plane of polarisation, by a surface properly inclined, *there is no partial reflection:* and in intermediate positions, the intensity of the reflection is nearly as the *square of the cosine* of the angular distance of the two planes.

4. A portion of the *transmitted* light is polarised in a *direction perpendicular* to that of the plane of refraction, so that none of this portion is reflected by a second surface parallel to the first; and when there are several parallel surfaces in succession, the whole of the transmitted light becomes at last so polarised, that none of it is partially reflected.

5. The same transverse polarisation will happen, in a *greater number* of transmissions, when the *angle differs* from that of complete polarisation: and in the same manner a second partial reflection, by a surface parallel to the first, will produce a more complete polarisation, when the first is imperfect.

6. A perfect polarisation in any new plane, by a partial reflection at the appropriate angle, completely *supersedes* the former polarisation; but a reflection or refraction void of any polarising effect, which may be called a *neutral* reflection or refraction, changes the direction of the plane of polarisation, according to Mr. Biot's experiments, into that of the image of the former plane, supposed to be formed by the action of the given surface.

7. The light *ordinarily refracted* by a doubling crystal in

the plane of the principal section of the crystal, passing through its axis, is *polarised in that direction:* the light *extraordinarily* refracted in the *transverse direction.*

8. Light *previously polarised* is transmitted by the ordinary refraction when its plane of polarisation coincides with the principal section, and by the extraordinary when it is perpendicular to it. In intermediate directions, the quantity of light transmitted by each refraction is, according to Malus, as the square of the cosine and sine of the angle formed by the planes, passing through the paths of the ray, and a line parallel to the axis in each crystal, supposing the species of refraction to be exchanged.

9. The rays of light ordinarily transmitted by doubling crystals appear in general to *retain their previous polarisation,* like rays transmitted through simple substances; but the extraordinary refraction polarises them, according to Biot, like a neutral reflection at a surface coinciding with the principal section; the new plane of polarisation taking the place of the image of the former.

10. Reflections at *metallic surfaces* are generally neutral with respect to polarisation: but in oblique planes they seem, according to some experiments of Malus, to mix or depolarise the light subjected to them.

Section XIV.—*Of the Laws of extraordinary Refraction.*

The extraordinary refraction of regular doubling crystals may be correctly determined in all circumstances, by means of the Huyghenian supposition of an undulation diverging in the form of a spheroid, from every point of the medium, the velocity in any given direction being always proportional to the corresponding diameter, so that the successive spheroidal surfaces remain always similar to each other. The relations of the angles of incidence and refraction may be calculated by finding the point, in which any of the spheroids, supposed to represent the forms of the elementary undulations, at a given instant, is touched by a plane passing through that point of the surface, at which the original beam of light would have arrived, at the same instant, through the external medium; it may also be

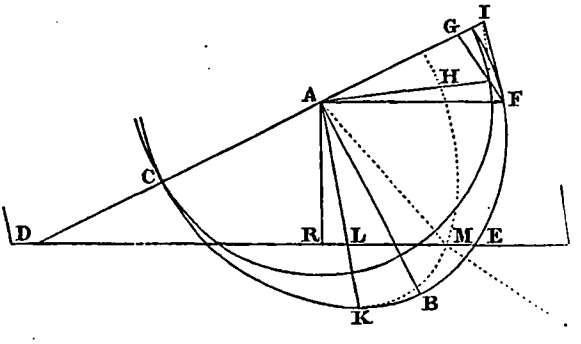
deduced, somewhat more simply, from the determination of the velocity with which an expanding spheroidal undulation must extend itself on any given surface: a velocity which immediately gives us the direction of the ray in the surrounding medium; and the relation thus obtained will also obviously hold good with respect to a ray returning in the opposite direction.*

In common refractions, if we compare the space described by an undulation on any given surface with the radius, the velocities appropriate to the different mediums will be represented by the sines of the respective angles. But the velocity, with which a spheroidal undulation advances on any surface, is evidently determined by the increment, or the fluxion, of the perpendicular to the circumference of the section of the spheroid, formed by that surface; and calling this perpendicular y, the velocity may be considered as proportional to its increment y': but the velocity in the surrounding medium is to that, with which the axis x increases, as r to 1, r being the index of the ordinary refractive density of the crystal, compared with that of the surrounding medium, since the velocity in the direction of the axis is the same as that which belongs to the ordinary refractive density; consequently, the increment of the path of the undulation in the surrounding medium will be expressed by rx', and s, the sine of refraction or incidence without the crystal, will be to the radius as rx' to y', and will be expressed by $\frac{rx'}{y'}$, or by $r\frac{dx}{dy}$, the evanescent increments of any quantities being always in the ratio of their fluxions: and the plane of refraction or incidence, without the crystal, will always be perpendicular to the tangent of the section formed by the refracting surface. The determination of the relation of the angles is therefore reduced to the calculation of the value of y and of its fluxion.

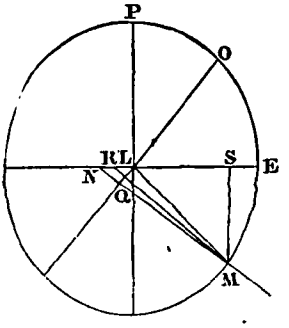
Supposing then the ratio of the greatest and least refractive densities of the crystal, or of the equatorial diameter of the spheroid 2AB to the axis 2AC to be that of n to 1, n being greater than unity, and the tangent of the angle ADE, formed

* Supra, No XIV., p. 263.

by the axis with the refracting surface DE, being called p; the magnitude of the semidiameter AF, parallel to the surface, may be found by comparing the secants of the angles FAG, HAG, subtended at the centre by the corresponding ordinates



of the ellipsis and the inscribed circle: for their tangents, FG, HG, being represented by p and $\frac{p}{n}$, the secants will be $\sqrt{(1+p^2)}$, and $\sqrt{\left(1+\frac{pp}{nn}\right)}$; and the semidiameter of the circle, AH, being x, that of the ellipsis, AF, will be $n\sqrt{\frac{1+pp}{nn+pp}}\,x$. But the tangent of the angle GIF, made by the tangent of the ellipsis with the axis, is to that of the angle made by the corresponding tangent of the circle, GIH or GHA, that is,



$\frac{n}{p}$, as n to 1; consequently, $\frac{nn}{p}$ will be the tangent of the angle made with the axis by the elliptic tangent, IF, or by the conjugate diameter AK; and if we substitute $\frac{nn}{p}$ for p, we shall find the length of this semidiameter AK $=\sqrt{\frac{n^4+p^2}{nn+pp}}\,x$, which is to that of the former AF in the ratio of $\frac{\sqrt{(n^4+p^2)}}{n\sqrt{(1+pp)}}$ to 1. Hence, for the lesser semiaxis of the section formed by the given surface, EL, calling AL the distance of its centre from that of the spheroid, z, we have the mean proportional between

the segments of the diameter $\sqrt{([AK + AL] \cdot [AK - AL])} = \sqrt{(AKq - ALq)} = \sqrt{\left(\frac{n^4 + p^2}{nn + pp} x^2 - z^2\right)}$, which must be reduced in the ratio of the semi-conjugate diameters AK and AF, so that it becomes $n \sqrt{\left(\frac{1 + pp}{nn + pp} x^2 - \frac{1 + pp}{n^4 + p^2} z^2\right)} = EL$. But from the known similarity of the parallel sections of a spheroid, the axes will be to each other as the semi-diameter $AF = n \sqrt{\frac{1 + pp}{nn + pp}} x$ is to nx the equatorial semi-diameter, a ratio which may be called that of 1 to m, m being $= \sqrt{\frac{nn + pp}{1 + pp}}$; so that the lesser axis EL being $= n \sqrt{\left(\frac{xx}{mm} - \frac{1 + pp}{n^4 + p^2} z^2\right)}$, the greater LP will be $n \sqrt{\left(x^2 - \frac{1 + pp}{n^4 + p^2} m^2 z^2\right)}$.

Now, if q be the cotangent of the angle MNE, formed by the plane of the ray's motion, in the external medium, with the lesser axis of the section, or the tangent of the angle ELO formed by the conjugate semidiameter LO with the same axis, this semidiameter may be found by substituting q for p, m for n, and the value of the semiaxis of the section for x, in the expression for AF, the semidiameter parallel to the refracting surface, and it becomes $m \sqrt{\frac{1 + qq}{mm + qq}} EL = n \sqrt{\frac{1 + qq}{mm + qq}\left(\frac{xx}{mm} - \frac{1 + pp}{n^4 + p^2} z^2\right)} = LO$. Hence, since all parallelograms described about an ellipsis are equal, dividing the product of the semi-axes EL.LP by this semidiameter, we shall have the required perpendicular $y = MQ = \frac{EL.LP}{LO} = \frac{LP}{m} \sqrt{\frac{mm + qq}{1 + qq}} = \frac{n}{m} \sqrt{\frac{mm + qq}{1 + qq}} \sqrt{\left(x^2 - \frac{1 + pp}{n^4 + p^2} m^2 z^2\right)}$. Now, in order to find the fluxion of this quantity, increasing as the spheroid increases, while the place of the centre of radiation remains unaltered, we must make z constant while x varies, and we shall have $dy = \frac{n}{m} \sqrt{\frac{mm + qq}{1 + qq}} x dx : \sqrt{\left(x^2 - \frac{1 + pp}{n^4 + p^2} m^2 z^2\right)}$, which must be equal to $\frac{r dx}{s}$; consequently, $\sqrt{\left(x^2 - \frac{1 + pp}{n^4 + p^2} m^2 z^2\right)} = \frac{ns}{mr} x \sqrt{\frac{mm + qq}{1 + qq}}$; and the lesser semiaxis of the section, EL, which

was found $= \frac{n}{m} \sqrt{\left(x^2 - \frac{1+pp}{n^4+p^2} m^2 z^2\right)}$, becomes $\frac{nns}{mmr} x \sqrt{\frac{mm+qq}{1+qq}}$; whence the semidiameter LM at the point of incidence, which may be called w, and which is analogous to the conjugate diameter AK in the former section, will be $\sqrt{\frac{m^4+q^2}{mm+qq}} \cdot \frac{nns}{mmr} \sqrt{\frac{mm+qq}{1+qq}} x = \frac{nns}{mmr} \sqrt{\frac{m^4+q^2}{1+qq}} x$. Hence it is obvious, that this semidiameter, in any one plane of incidence, will be in a constant proportion to the sines, as Huyghens himself demonstrated: so that, supposing x to be constant, and z to vary, the semidiameter w may be considered as an ordinate in the elliptic section passing through the point of incidence M and the diameter AK conjugate to the refracting surface, which is also the path of a ray falling perpendicularly on that surface from without: and the tangent of the angle ELM formed by this semidiameter with the lesser axis of the given section, will be $\frac{mm}{q}$, which determines the intersection of this oblique plane with the refracting surface.

But in order to find the angle made with the refracting surface in a plane perpendicular to it, we must compute LR, the distance of the centre of the refracting section from the point nearest to the centre of the spheroid: and the tangents of the inclinations of the diameters to the axis being p and $\frac{nn}{p}$, that of their mutual inclination will be $\frac{nn+pp}{p(1-nn)}$, since $\tan(a+b) = \frac{\tan a + \tan b}{1 - \tan a \tan b}$; and the sine of the same angle being expressed by $\frac{\tan a + \tan b}{\sec a \sec b}$, it becomes here $\frac{nn+pp}{\sqrt{(1+p^2)}\sqrt{(n^4+p^2)}}$ = sin FAK = sin ALR, which we may call τ, and the cosine $\frac{1-\tan a \tan b}{\sec a \sec b}$ $= \frac{p(1-nn)}{\sqrt{(1+p^2)}\sqrt{(n^4+p)}} = t$: and the required distance LR will be tz, and the distance of the centre of the spheroid from the refracting surface AR $= \tau z$. But MS, the perpendicular falling, from the point of incidence, on the lesser axis of the section formed by the surface, being called u, the tangent of the angle MLS, subtended by it at the centre being $\frac{mm}{q}$, and

its sine consequently $\frac{mm}{\sqrt{(m^4+q^2)}}$, we have $u = \frac{mmw}{\sqrt{(m^4+q^2)}} = \frac{n^2sx}{r\sqrt{(1+q^2)}}$; and the distance of this perpendicular from the centre, LS $= v = \frac{qw}{\sqrt{(m^4+q^2)}}$; or if we call the sine of ordinary refraction $\frac{s}{r} = \rho$ and the sine of the inclination of the plane of the ray's motion to the lesser axis, $\frac{1}{\sqrt{(1+qq)}}$, h, and its cosine $\frac{q}{\sqrt{(1+qq)}} = h$, we have $u = n^2\rho hx$, and $v = \frac{nnq}{mm}\rho hx$. Hence the cotangent of the angle ERM, formed by the line nearest to the ray in the section with the lesser axis, will be $\frac{v+tz}{u}$, if the value of s be considered as positive, when the ray is inclined on the refracting surface towards the axis of the crystal; for in this case the sign of t being negative, tz or LR will be subtracted from v or LS; and the reverse when s is negative. We have also for the hypotenuse RM, or the distance of the point of incidence from the point nearest to the centre of the spheroid, $\sqrt{(u^2 + [v + tz]^2)}$; consequently the tangent of RAM, the angle of incidence or refraction within the crystal, will be $\frac{\sqrt{(u^2+[v+tz]^2)}}{\tau z}$. Now since it has been shown that $\sqrt{(x^2 - \frac{1+pp}{n^4+p^2}m^2z^2)} = \frac{ns}{mr}\sqrt{\frac{mm+qq}{1+qq}}\,x$, we have $z^2 = \frac{n^4+p^2}{mm(1+pp)}\left(1 - \frac{nnss}{mmrr}\cdot\frac{mm+qq}{1+qq}\right)x^2$, and the cotangent of the inclination of the plane of refraction ERM, or $\frac{v+tz}{u} = \frac{q}{mm} + \frac{tz}{u}$, becomes $\frac{q}{mm} + \frac{p(1-nn)}{m(1+pp)}\sqrt{\left(1 - \frac{nn}{mm}[m^2k^2+k^2]\rho^2\right)}\cdot\frac{1}{nn\rho k}$; and since $\tau^2z^2 = (m^2 - n^2\rho^2\,[m^2k^2+k^2]\,x^2$, the tangent of the angle of incidence or refraction within the crystal, which is $= \sqrt{\left(\frac{uu}{\tau\tau zz} + \frac{vv}{\tau\tau zz} + \frac{2t}{\tau}\cdot\frac{v}{\tau z} + \frac{tt}{\tau\tau}\right)}$ will be represented by $\sqrt{\left(\frac{n^4m^4k^2+n^4k^2}{m^4(m^2-n^2\rho^2[m^2k^2+k^2])}\rho^2 + \frac{2p(1-nn)nnk\rho}{(nn+pp)mm\sqrt{(m^2-n^2\rho^2[m^2k^2+k^2])}} + \left[\frac{p(1-nn)}{nn+pp}\right]^2\right)}$. The value of the perpendicular to the surface, AR or τz, is also of importance as immediately indi-

cating, by its proportion to the axis x, the velocity of the undulation in the direction of the depth, which is therefore represented by $\sqrt{(m^2 - n^2\rho^2[m^2k^2 + h^2])}$.

These expressions become somewhat simpler in many cases of common occurrence; thus, when the axis is parallel to the surface, $p = 0$, $m = n$, and $t = 0$, consequently, the tangent of refraction is $\frac{\varrho}{n}\sqrt{\frac{n^4k^2 + h^2}{1 - (nnkk + hh)\rho\rho}}$, and the perpendicular velocity $n\sqrt{(1 - [n^2k^2 + h^2]\rho^2)}$. When the axis is perpendicular to the surface, p is infinite, $m = 1$, and t is again $= o$; and the tangent of the angle of refraction is $\frac{nn\varrho}{\sqrt{(1 - nn\varrho\varrho)}}$, the perpendicular velocity being $\sqrt{(1 - n^2\rho^2)}$.

The retardation, produced by the passage of light through such a plate, being equal to the time occupied within the plate, diminished by a time proportional to the product of the tangent of the angle of refraction and the sine of the angle of incidence (see Section V.); it will be expressed in the case of a plate parallel to the axis, by $\frac{r}{n\sqrt{(1 - [nnkk + hh]\varrho\varrho)}} - s\rho\sqrt{\frac{n^4k^2 + h^2}{1 - (nnkk + hh)\rho\rho}}$; and when the axis is perpendicular to the plate, by $\frac{r}{\sqrt{(1 - nn\rho\rho)}} - \frac{snn\varrho}{\sqrt{(1 - nn\varrho\varrho)}} = \frac{r(1 - nn\varrho\varrho)}{\sqrt{(1 - nn\rho\rho)}} = r\sqrt{(1 - nn\rho\rho)}$. The effect of any small change in the form of the spheroid, on the retardation, may be found from the fluxions of these quantities, supposing n to vary; which when properly reduced, making $n = 1$, will be $-r\frac{1 - hh\varrho\varrho}{\sqrt{(1 - \varrho\varrho)}}\,dn$, and $-\frac{r\varrho\varrho}{\sqrt{(1 - \varrho\varrho)}}\,dn$ respectively.

The values of r and n, for the principal substances, exhibiting the extraordinary refraction, which have been examined, are these:—

Iceland crystal . .	$r = 1.657$	$n = 1.1140$	$= 10 : 9$
Arragonite . . .	1.693	1.1030	$= 11 : 10$
Ice	1.310	.9989	$= 890 : 891$
Quartz	1.558	.99444	$= 179 : 180$
Sulfate of lime . .	1.525	.99432	$= 175 : 176$
Sulfate of barita .	1.635	.99295	$= 142 : 143$

In mica, according to Mr. Biot, and in arragonite, according to Dr. Brewster, there are two axes of crystallization; and the refraction of such substances may probably be represented, by supposing all the circular sections of a spheroid to become ellipses, so that the undulation may assume the shape of an almond.

SECTION XV.—*Of the Colours of doubly refracting Substances.*

In the case of doubly refracting substances, the first difficulty is, not to explain why the colours of double lights are sometimes produced, but why they are not more universally observable; since it might naturally be expected, as a consequence of the general law of interference, that two portions of the same beam, passing through a moderately thin plate of such a substance, in paths differing but little from each other, and coinciding again in direction, should, in all common cases, exhibit colours nearly similar to those of ordinary thin plates. It would, however, be difficult to conjecture, whether they ought to resemble the colours seen by transmission or by reflection; and the fact is, that both these series of colours are at once produced by the substances in question; but they are so mixed, that, without a particular arrangement, they always neutralise each other; and their formation appears to be also limited to certain peculiar conditions of polarisation, consistent with Mr. Arago's observation, on the non-interference of two portions of light, polarised in transverse directions. Several of the cases, indeed, in which they are exhibited, remain still involved in some degree of obscurity; but it is easy to analyse the most important of the phenomena, and to reduce them, with great precision, to the general laws of periodical colours.

Mr. Malus has demonstrated, by satisfactory experiments, that a beam of light, admitted into a doubly refracting crystal, is as much divided by partial reflection at the second surface, as by transmission at the first: the directions and the relative intensities of the two portions being precisely the same as those of the two portions of ray similarly polarised, and returning to the second surface from without in an equal angle; so that after a farther transmission at the first surface, all the portions

become again parallel. When the ray is in the direction of the principal section, there is no separation, each of the pencils proceeding undivided, as they would do if they passed through a second crystal parallel to the first: and the separation becomes the most complete when the plane of incidence makes an angle of about 45° with the principal section; each of the portions *o* and *e*, into which the ray is divided upon its admission, affording them two reflections, *oO* and *oE*, *eO* and *eE*, of nearly equal intensity. The times occupied by the portions *oO*, *eE*, will differ most from each other, while *oE* and *eO* will describe their paths in equal times of intermediate length: but of these *eO* only will commonly interfere with *oO*, which has a similar polarisation in the plane of incidence, and *oE* with *eE*, both being polarised in a transverse direction; so that we have two series of colours, depending on an equal interval, except so far as they are distinguished by the inversion of one of the portions belonging to the extraordinary reflection, which renders the series of colours exhibited by them similar to that of the colours of common thin plates seen by reflection, while the ordinary reflection exhibits colours analogous to those of thin plates seen by transmission.

Mr. Biot's usual mode of exhibiting these colours is to place a thin plate of sulfate of lime, or of any other crystal, on a black substance; to allow it to reflect the white light of the clouds at an angle of incidence of about 55°; and to receive this light on a black glass, at an equal angle of incidence, in a plane transverse to the former, so that the plate may be viewed by reflection in the black glass. In this arrangement the light reflected from the upper surface of the plate, being polarised in the first plane of reflection, is not reflected by the black glass, and consequently is incapable of rendering the colours less easily perceptible by admixture with them: the beams *oO* and *eO*, returning by the ordinary reflection, are also similarly polarised, and will be transmitted or absorbed by the glass; but the beams *oE* and *eE*, being polarised in a transverse direction, will be partially reflected by it, and will exhibit a very brilliant colour, depending on their mutual interference. If, on the contrary, the black glass be turned round the ray, so that the

second plane of incidence may coincide with the first, the ordinary rays only will be partially reflected by it, and the complementary colour will be exhibited by the union of the portions oO, eO; but this colour will be less distinct, on account of its mixture with the white light reflected by the first surface.

Appearances of a similar nature may also be observed in the transmitted light; each of the refractions exhibiting the colour complementary to that which it affords by reflection, as happens in the ordinary colours of thin plates; and we must seek for the portions of light which afford them, in the successive partial reflections at the two surfaces of the plate, as in the case of the ordinary colours; the light simply transmitted by the separate refractions not exhibiting the ordinary effects of interference, for want of a similarity of polarisation. The obliquity of the incident light produces similar effects on both series.

Under some circumstances of the reflection of rays near the perpendicular, Mr. Biot observes that the plate assumes the colour which is usually exhibited by a plate of twice the thickness viewed a little more obliquely: and in such cases it is probable that the polarisation of the beams oO and eE has been so modified as to afford a partial interference; and if this is not the true explanation, it will not be difficult to suppose the interval to be doubled in some other manner by a repeated reflection.

The effect of a plate of a double thickness is also produced by two equal and parallel plates, through which the light passes in succession, provided that their axes of crystallization be parallel, and that they be of such a thickness, as to exhibit in conjunction a colour more easily observable than those which they afford separately: a condition which is more generally applicable to the case in which the axes are transverse to each other, and one of the thicknesses is to be subtracted from the other; since in this situation the two portions of light must always interchange their refractions, and that which has moved the more slowly, in its passage through one of the plates, will move the more rapidly in the other. This result is very accurately confirmed by experiment, and certainly affords a very striking illustration of the truth of the law of interference.

When we wish to examine the effects of the different obliquities of the incident light, it is most convenient to employ a beam previously polarised, which renders the separation of the different portions by a subsequent reflection or refraction more easily practicable: and for these purposes we may either make use of plates of black glass, placed in proper situations, or polarising piles, consisting of a number of oblique thin plates, which produce the effect on the light transmitted through them, with less diminution of its intensity than would take place in a single partial reflection. In some cases also, the light may be analysed by causing it to pass through a piece of Iceland crystal; or through a thin plate of agate, which Dr. Brewster has found to transmit only such light as is polarised in a particular plane.

The measurements of the thickness of plates of doubly refracting substances agree in general very accurately with the various tints exhibited by them in various situations with respect to the axis, and with various obliquities of the incident light, according to the theory of periodical colours; and the agreement is always sufficiently perfect to convince us of the dependence of the phenomena on the law of interference, even if it should happen to require some unknown modification in particular cases. In the first place, when the incidence is perpendicular, the thickness of the plates is precisely such as would be inferred from the theory, at least as nearly as the theory is founded on observations sufficiently accurate, although this thickness is often many hundred times as great as that of the thin plates, with which it is to be compared: thus the greatest disproportion of the ordinary and extraordinary refraction of rock crystal, according to Malus's experiments, is that of 159 to 160; so that the difference of the times, occupied by light in passing through this substance, is to the interval, in virtue of which a similar plate exhibits the common colours, as 1 to 320, and to the interval in a plate of crown glass as 1 to 318; while the experiments of Mr. Biot make the observed proportion that of 1 to 360; the difference being no greater, than would arise from an error of less than a thousandth part of the whole, in the determination of one of the refractive densities.

The effect of the obliquity of the incident light, on the colours exhibited by plates of rock crystal, agrees also perfectly with the theory. The difference of the times required for the ordinary and extraordinary refractions, which is always comparatively small, will vary as the fluxion of the retardation, when the obliquity varies; and the sine of ordinary refraction being ϱ, the interval will be expressed by $-r\frac{1-hh\varrho\varrho}{\sqrt{(1-\varrho\varrho)}}\,dn$, when the axis is parallel to the surface of the plate, and by $-r\frac{\varrho\varrho}{\sqrt{(1-\varrho\varrho)}}\,dn$, when it is perpendicular. Taking, for example, an experiment of Mr. Biot, on a plate in which the axis was nearly perpendicular, the mean angle of refraction being 21° 38.5′, the tint was a reddish white of the seventh order, answering to the reflection from a plate of glass .0000496 of an inch thick, in the experiments of Newton, while the colour exhibited in a perpendicular light, by a plate of the same crystal, in which the axis was parallel to the surface, would have been expressed by the thickness .000332. In these two cases, the values of the fluxion become $-rdn$ and $-.14633rdn$; and reducing the interval .000332 in this proportion, we find .000046 for the thickness of a plate of glass which ought to exhibit the tint corresponding to the oblique incidence; the difference from the experiment being only one millionth of an inch, which would scarcely make a sensible alteration in the colour observed. When the thickness of such a plate is more considerable, or when the excentricity of the extraordinary refraction is greater, the colours differ, with the incidence, in different parts of the plate; and they are generally disposed in rings concentric with the axis. These rings have been particularly described by Dr. Brewster, as observed in the topaz: they are always interrupted by a dark cross, occasioned by the want of light, properly polarised to afford them, in the two transverse directions.

Mr. Biot has made a great number of experiments on the colours of the plates of sulfate of lime, in the form denominated Muscovy talc: they exhibit a general agreement with the results of the calculation, particularly with respect to the constancy of the tint, in all moderate obliquities, when the inclination of the axis to the plane of incidence is 45°; but in other cases the

agreement is somewhat less perfect, and the difference is too great to be attributed altogether to accident. The most probable reason for this irregularity, under circumstances so nearly similar to those which accord with the theory in the case of rock crystal, is the want of a perfect identity of the two refractions, in the direction of the supposed axis; or, in the language employed by Mr. Biot with respect to mica, the existence of a double axis of extraordinary refraction: and it is the more credible that such a slight irregularity may have existed in the sulfate of lime without having been observed, as Dr. Brewster has lately detected a similar property in the arragonite, though both Malus and Biot had examined this substance very carefully without being aware of it. The calculation of the extraordinary refraction, in such a case, would afford but little additional difficulty, if its characters were well determined: the form, in which the undulations must be supposed to diverge, might properly be termed an *amygdaloid;* and the velocities with which the sections, formed by the given surface, would extend themselves, might be deduced from the properties of the ellipsis, nearly in the same manner as they have been determined for the spheroid.* The difference of the results of the calculation from the spheroid, and of Mr. Biot's experiments, or rather of the empirical formula, derived from them, may be seen in the subjoined table; the first part of which, deduced from the theory, is applicable to all substances affording a regular extraordinary refraction, when the axis is either perpendicular or parallel to the surface of the plate. The first column of decimals shows the equivalent variation of thickness where the axis is perpendicular to the plate, being equal to $\frac{\varrho\varrho}{\sqrt{(1-\varrho\varrho)}}$, the product of the sine and tangent of refraction; the second represents the variation for an ordinary thin plate, being proportional to the cosine $\sqrt{(1-\varrho\varrho)}$; and the subsequent columns are found by adding to the numbers of the second column those of the first, multiplied by k^2, the square of the

* This was written before Fresnel's important investigation of the *surface of elasticity* and more complete development of the theory of polarised light, which was presented to the Académie des Sciences in 1821. The speculations in the text, imperfect as they are, and in some important respects erroneous, are remarkable anticipations of the true theory.—*Note by the Editor.*

sine of the inclination of the plane of incidence to the axis, since $\frac{1 - hh\varrho\varrho}{\sqrt{(1-\varrho\varrho)}} = \sqrt{(1 - \varrho\varrho)} + \frac{kk}{\sqrt{(1 - \varrho\varrho)}}$.

Angle of Refraction.	Perpendicular Plate.	Parallel Plate. Inclination of the Plane of Incidence to the principal Section.				
			22½°	45°	77½°	90°
00°	.0000	1.0000	1.000	1.000	1.000	1.000
20	.1245	.9397	.958	1.002	1.046	1.064
40	.5394	.7660	.845	1.036	1.245	1.305
60	1.5000	.5000	.720	1.250	1.780	2.000
80	5.5851	.1736	.992	2.966	4.940	5.759
Biot—						
20		.969	.975	.995	1.023	1.038
40		.898	.920	1.000	1.112	1.175
60		.848	.882	1.097	1.396	1.588
80		1.196	.921	1.440	2.338	3.562

There are also some circumstances in the experiments of Mr. Biot on plates of rock crystal cut perpendicularly to the axis, which cannot be sufficiently explained on any hypothesis, without some further investigation. These plates seem to transmit the beam of light subjected to the experiment, without materially altering its polarisation, and then to produce different colours, according to the situation of the substance subsequently employed for analysing the light: so that Mr. Biot supposes the rays of light to be turned more or less by the crystal, round an axis situated in the direction of their motion; and he has observed some similar effects in oil of turpentine, and in some other fluids. But it is highly probable, that all these phenomena will ultimately be referred to some simpler operation of the general law of interference.

Dr. Seebeck and Dr. Brewster have discovered appearances of colours, like those of doubly refracting substances, in a number of bodies which can scarcely be supposed to possess any crystalline structure. They are particularly conspicuous in large cubes of glass, which have been somewhat suddenly cooled, so that their internal structure has been rendered unequal with regard to tension. The outside of a round mass, thus suddenly cooled, being too large for the parts within it,

must necessarily be held by them in a state of compression with respect to the direction of the circumference, while they are extended in their turn by its resistance: although in the direction of the diameter the whole will generally be in a state of tension: so that the refractive density may naturally be expected to be somewhat different in different directions, which constitutes the essential character of oblique refraction: and when the proportions of the external parts to the internal are modified by the existence of angles, or other deviations from a spherical form, the arrangement of the tensions must be altered accordingly; and there is no doubt that all the apparently capricious variations of the rings and bands of colours which are observed, might, by a careful and minute examination, be reduced to the natural consequences of these inequalities of density, so far at least as the laws of the extraordinary refraction alone are concerned, although the separation of the light into two portions might still remain unexplained. Effects of the same kind are produced by the temporary operation of partial changes of temperature, producing partial compression and extension of the internal structure of the substance: and even a mechanical force, if sufficiently powerful, when applied externally in a single direction, has been shown, by the same observers, to produce a double refraction; although the difference of the densities, thus induced, is much too minute to be perceived in any other way, than by means of these colours, which are, in general, so much the more easily seen, as the cause which excites them is the feebler.

Dr. Brewster has also shown, that the total reflection of light within a denser medium, and the brilliant reflection at the surfaces of some of the metals, are capable of exhibiting some of the appearances of colour; as if the light concerned were divided into two portions, the one partially reflected in the first instance, the other beginning to be refracted, and caused to return by the continued operation of the same power. In the case of silver and gold, it has already been observed, that there appear to be two kinds of reflection, occasioning opposite polarities; and these may possibly be concerned in the production of this phenomenon. The original interval appears to be extremely

minute, but it is capable of being increased by a repetition of similar reflections, as well as by obliquity of incidence. Mr. Biot has also found that such surfaces, combined with plates of doubly refracting substances, either increase or diminish the equivalent thickness, according to the direction of the polarisation which they occasion. In these and in a variety of similar investigations, a rich harvest is opened, to be reaped by the enlightened labours of future observers; and the more difficulty we find in fully explaining the facts, upon the general principles hitherto established, the more reason there is to hope for an extension of the bounds of our knowledge of the optical properties of matter, and of all the laws of nature connected with them, when the examination of these apparent anomalies shall have been still more diligently pursued.

Section XVI.—*Of the Nature of Light and Colours.*

Notwithstanding the acknowledged impossibility of fully explaining all the phenomena of light and colours by any imaginable hypothesis respecting their nature, it is yet practicable to illustrate them very essentially, by a comparison with the known effects of certain mechanical causes, which are observed to act in circumstances somewhat analogous; and as far as a theory will enable us to connect with each other a variety of facts, it is perfectly justifiable to employ it hypothetically, as a temporary expedient for assisting the memory and the judgment, until all doubts are removed respecting its actual foundation in truth and nature. Whether, therefore, light may consist merely in the projection of detached particles with a certain velocity, as some of the most celebrated philosophers of modern times assert, or whether in the undulations of a certain ethereal medium, as Hooke and Huyghens maintained, or whether, as Sir Isaac Newton believed, both of these causes are concerned in the phenomena; without positively admitting or rejecting any opinion as demonstrably true or false, it is our duty to inquire what assistance can be given to our conception and recollection, by the adoption of any comparison, which may be pointedly applicable even to some insulated facts only. It has

however been thought desirable to separate this investigation, as much as possible, from the relation of the facts, in order to avoid confounding the results of observation with the deductions from mere hypothesis; an error which has been committed by some of the latest and most meritorious authors in this department. It may be objected to some of the preceding sections, that this forbearance has not been exercised with respect to the general law of interference and its modifications: but it would have been impossible to give any correct statement of the facts in question, without determining whether the appearances depend upon one or both of the portions of light supposed to be concerned.

Art. 1. (Sect. I.) The separation of colours is explained, in the hypothesis of emission, by the supposition of an elective attraction, different in intensity for the different rays of the spectrum; but for this difference no ulterior cause is assigned. Any original difference of velocity is contrary to direct experiment; and even the alterations of relative velocity, which must inevitably be occasioned by a variety of astronomical causes, have not been detected by the most accurate observations, instituted for the express purpose of discovering them; so that it has been suggested, that there may possibly be a multitude of rays of the same colour, moving with various velocities, and only affecting the sense when they have the velocity appropriate to that colour in the eye. The name of elective attraction is indeed little more than a mode of expressing the fact, without referring it to any simpler mechanical cause; and in chemical elective attractions the substances concerned are under very different circumstances, with respect to contact, and with respect to the probable influence of the form and bulk of their integral particles; at the same time it seems impossible to show any absurdity in the supposition of the existence of such an elective attraction with regard to the different kinds of light. On the other hand, if we consider colours as depending on a succession of equal undulations, of different magnitudes as the colours are different, we may discover an analogy, somewhat more approaching to a mechanical explanation, in the motions of waves on the surface of a liquid; the largest waves moving

with the greatest rapidity, although the approximate calculation, derived from the most approved theory, leads us to the same expressions for the velocity, as are applicable to the transmission of an impulse through an elastic fluid. The fact is, that a larger wave moves more rapidly than a smaller, because the pressure is not precisely limited to a perpendicular direction, as the simplest calculation supposes, but operates also more or less in an oblique direction, principally within a certain angular limit; so that the utmost depth at which any difference of pressure can affect the liquid as a motive force, is that at which this angle may be imagined to comprehend virtually the exact breadth of a wave; and since the velocity depends on the depth of the fluid affected at once by the pressure, the breadth becomes in this manner an element of the determination. Thus also the larger undulations, constituting red light, are found to move more rapidly than those of the violet, which are supposed to be smaller; and there are many ways in which the difference may be supposed to be occasioned, although not depending exactly on the same cause as in the case of the waves on the surface of a liquid. It is well known that sounds of all kinds move with an equal velocity through the air; and all colours arrive through the supposed elastic ether in the same time from the remotest planets: but a refractive medium, however transparent, is not to be considered as perfectly homogeneous: in many instances, two mediums, of different qualities, seem to pervade every part of a crystal, which is completely uniform in its appearance; and it seems to be necessary, in every case, to suppose the particles of material bodies scattered at considerable distances through a medium which passes freely through their interstices; so that we may conceive the undulations of light to be transmitted partly through the particles themselves, and partly through the intervening spaces, the two portions meeting continually after a certain very minute difference in the length of their paths: we may then suppose the portion transmitted through the interstices to be weakened by the irregularity of its passage, which will affect the smaller undulations more than the larger; and when these portions are combined with the portions more slowly transmitted through the particles themselves, these

last will bear a greater proportion to the former in the violet than in the red light, and will have more influence on the ultimate velocity, which will therefore be smaller for the violet than for the red. This explanation may perhaps be far from the best that the hypothesis in question might afford; but it will serve as an illustration of a possible mode, in which the phenomenon may be referred to the established laws of mechanics, without the continual introduction of new principles and properties.

Art. 2. (Sect. IV. A.) Most of the ordinary phenomena of optics are capable of a sufficiently satisfactory explanation, on either of the hypotheses respecting the nature of light and colours: but the laws of interference, which have been shown to be so extensively applicable to the diversified appearances of periodical colours, point very directly to the theory of undulation; so directly, indeed, that their establishment has been considered, by many persons on the continent, as almost paramount to the establishment of that theory. It might not, however, be absolutely impossible, to invent some suppositions respecting the effects of light, which might partially reconcile these laws to the theory of emission. Thus, if we suppose, with Newton, the projected corpuscles of light to excite sensation by means of the vibrations of the fibres of the retina and of the nerves, we may imagine that such vibrations must be most easily produced by a series of particles following each other at equal distances, each colour having its appropriate distance in any given medium: it will then be demonstrable, that any second series of similar particles, interfering with them, in such a manner as to bisect their intervals, will destroy their effect in exciting a vibratory motion; each succeeding particle meeting the fibre at the instant of its return from the excursion occasioned by the stroke of the preceding, and thus annihilating the motive effect of that stroke. But the illustration ends here: for it seems impossible to adapt it to the greater number of the alternations which occur, during the passage of a ray, through a given space, in a denser medium: since it is an indispensable condition of the projectile theory, that the velocity of light should be increased upon its entrance

into a medium of greater refractive density. The Newtonian theory, of fits of easy reflection and easy transmission, is still more limited in its application; since it attributes to one portion of light those effects, which have been strictly demonstrated to depend on the presence of two.

In the undulatory theory, the analogy between the laws of interference, and the phenomena of the tides, and the effects of the combination of musical sounds, is direct and striking. The existence of an undulation of an elastic medium depends on the recurrence of opposite motions, alternately direct and retrograde, at certain equal distances, in the same manner as a series of waves consists in a number of alternate elevations and depressions, and the succession of the tides in a number of periods of high and low water. The spring and neap tides, derived from the combination of the simple solar and lunar tides, afford a magnificent example of the interference of two immense waves with each other: the spring tide being the joint result of the combination when they coincide in time and place, and the neap when they succeed each other at the distance of half an interval, so as to leave the effect of their difference only sensible. The tides of the port of Batsha, described and explained by Halley and Newton, exhibit a different modification of the same opposition of undulations; the ordinary periods of high and low water being altogether superseded, on account of the different lengths of the two channels by which the tides arrive, affording exactly the half interval which causes the disappearance of the alternation. It may also be very easily observed, by merely throwing two equal stones into a piece of stagnant water, that the circles of waves, which they occasion, obliterate each other, and leave the surface of the water smooth, in certain lines of a hyperbolic form, while, in other neighbouring parts, the surface exhibits the agitation belonging to both series united. The beating of two musical sounds, nearly in unison with each other, appears also to be an effect exactly resembling the succession of spring and neap tides, which may be considered as the beatings of two undulations, related to each other in frequency as 29 to 30; and the combination of these sounds is still more identical with that which this theory

attributes to light; since the elementary motions of the particles of the luminiferous medium are supposed to be principally confined to the line of direction of the undulation, while the most sensible effects of the waves depend immediately on their ascent and descent, in a direction perpendicular to that of their progressive motion.

Art. 3. (Sect. IV. B.) The diminution of the velocity of light upon its entrance into a denser medium, in the direct proportion of the refractive density, is one of the fundamental principles of the undulatory theory, and is perfectly inadmissible on the supposition of projected corpuscles. But it must be remembered, that the demonstration of the actual existence of this proportion is somewhat indirect; being only derived from the necessity of admitting it, in the application of the laws of interference to the observed phenomena: and we have no means of obtaining an immediate measure of the velocity of light in different mediums.

Art. 4. (Sect. IV. C.) The loss of the half interval may be explained in particular cases, without difficulty, although, in other instances, the circumstances are too complicated to allow us to appreciate their effects. In the direct transmission of a ray of light through a plate of a transparent substance, we may compare the denser medium to a series of elastic balls, larger and heavier than another series in contact with them on each side. Now, it is well known that a series of elastic balls transmits any motion from one end to the other, while each ball remains at rest, after having communicated the motion to the next in order; so that the last only flies off, from having none beyond it to impel; and if the balls, instead of being only possessed of repulsive forces, were connected by elastic ligaments of equal powers, a motion in a contrary direction would be transmitted with equal ease; the last ball, being retained by the ligament, instead of flying off, would draw the last but one in the same direction, itself remaining at rest after this negative impulse; and the motion would be communicated backwards in the same manner throughout the series to the first ball: and then, for want of further resistance, this ball would not remain at rest after receiving the negative impulse, but would be

drawn forwards by it, so as to strike the second, precisely in the same manner as at the beginning of the experiment; and this second positive impulse would proceed through the whole series like the first. Such is the nature of the longitudinal vibrations of elastic rods, first observed by Chladni; the cohesion of the substance supplying the place of the supposed elastic ligaments: and in the case of an elastic fluid, the pressure of the surrounding parts performs the same office; a negative impulse being always propagated through it with the same facility as a positive one. If, instead of a single series of balls, we now consider the effect of two series, the second consisting of larger balls than the first, the last ball of the smaller series will not remain at rest after striking the first of the larger, but will be reflected, so as to strike the last ball but one in a retrograde direction; and this retrograde impulse will be continued to the first ball, constituting a positive impulse with respect to the new direction, in which it is propagated. But if the first series of balls be larger than the second, the last of the larger balls will not be deprived of all its motion by striking the first of the smaller, but will continue to move more slowly in its first direction; and the elastic ligaments will then be called into action, so as to carry back, step by step, to the first ball, this remaining impulse, which will become negative with respect to the new direction of its transmission. And the same must happen in the case of two elastic mediums in contact, supposing them to be of equal elasticity, but of different densities; the direction of the elementary motions either coinciding with that of the general impulse, or being opposite to it in both mediums at once, when the reflection is produced by the arrival of the undulation at the surface of a denser medium, and being reversed when at the surface of a rarer: and it is obvious that such an inversion of any regular undulation is paramount to its retardation or advancement, to the extent of half of the interval which constitutes its whole breadth; every affection of such an undulation being precisely inverted, at the distance of half the breadth of a complete alternation: and these effects will not materially differ, whether the impulse be supposed to arrive perpendicularly at the surface, or in an oblique direction.

Art. 5. (Sect. IV. D.: Sect. XIII.) The experiments of Mr. Arago, not yet published in detail, which show that light does not interfere with light polarised in a transverse direction, lead us immediately to the consideration of the general phenomena of polarisation, which cannot be said to have been by any means explained on any hypothesis respecting the nature of light. It is certainly easier to conceive a detached particle, however minute, distinguished by its different sides, and having a particular axis turned in a particular direction, than to imagine how an undulation, resembling the motion of the air which constitutes sound, can have any different properties, with respect to the different planes which diverge from its path. But here the advantage of the projectile theory ends; for every attempt to reduce the phenomena of polarisation to mechanical laws, by the analogy of magnetism, has completely failed of enabling us to calculate the results of the actions of the forces, supposed to be concerned, in any correct manner; to say nothing of the extreme complication of the properties, which it would be necessary to attribute to the simplest and minutest substances, in order to justify the original hypothesis of a polarity, existing in all the particles of light, and a directive attraction, that is, a combination of attraction and repulsion, in every reflecting or refracting substance. In the undulatory theory, we may discover some distant analogies, sufficient to give us a conception of the possibility of reconciling the facts with the theory, and perhaps even of reducing those facts to some general laws derived from it; although it will be necessary, in this intricate part of the inquiry, to proceed analytically rather than synthetically, and to rest satisfied for the present, without bringing the analysis to a termination by any means explanatory of all the phenomena. Some of the supporters of this theory may perhaps be of opinion, that its deficiencies are too strongly displayed by this attempt: but it is for them to find a more complete solution of the difficulties, if any such can be discovered.

In the case of a wave, moving on the surface of a liquid, considering the motion of the particles at some little distance below the surface, as concerned in the propagation of an undulation in a horizontal direction, we may observe that there is

actually a lateral motion, throughout the liquid, in a plane of which the direction is determined by that of gravitation: but this happens because the liquor is more at liberty to extend itself on this side than on any other, the force of gravitation tending to bring it back, with a pressure of which the operation is analogous to that of elasticity; and we cannot find a parallel for this force in the motions of an elastic medium. It is indeed very easy to deduce a motion, transverse to the general direction, from the combination of two undulations proceeding from two neighbouring points, and interfering with each other, when the difference of their paths amounts to half an interval; for the result of this combination will be a regular though a very minute vibration in a transverse direction, which will continue to take place throughout the line of the propagation of the joint motions although certainly not with any force, that would naturally be supposed capable of producing any perceptible effects. There must even be a difference in the motions of the particles in every simply diverging undulation, in different parts of the spherical surface to which they extend: for, supposing it to originate from a vibration in a given plane, the velocity of the motion constituting the undulation will be greatest in the direction of that plane, and will disappear in a direction perpendicular to it, or rather will there become transverse to the direction of the diverging radii: and in all other parts there must be a very minute tendency to a transverse motion, on account of the difference of the velocities of the collateral direct motions, and of the compressions and dilatations which they occasion. When, also, a limited undulation is admitted into a quiescent medium, it loses some of its force by diffraction on each side, where it is unsupported by the progress of the collateral parts: and if an undulation were admitted by a number of minute parallel linear apertures or slits, or reflected from an infinite number of small wires, parallel to each other, it would still retain the impression of the incipient tendency to diffraction in all its parts, producing a modification of the motion, in a direction transverse to that of the slits or wires. It is true that all these motions and modifications of motion would be minute beyond the power of

imagination, even when compared with other motions, themselves extending to a space far too minute to be immediately perceived by the senses: and this consideration may perhaps lessen the probability of the theory as a physical explanation of the facts: but it would not destroy its utility as a mathematical representation of them, provided that such a representation could be rendered general, and reducible to calculation; and even in a physical sense, if the alternative were unavoidable, it is easier to imagine the powers of perceiving minute changes to be all but infinite, than to admit the portentous complication of machinery, which must be heaped up, in order to afford a solution of the difficulties, which beset the application of the doctrine of simple projection to all the phenomena of polarisation and of colours. It is not however possible at present to complete such a mathematical theory, even on imaginary grounds; although a few further analogies between polarisation and transverse motion force themselves on our observation.

In the theory of emission, the resemblance of the phenomena of polarisation to the selection of a certain number of particles, having their axes turned in a particular direction, supposing these axes, like those of the celestial bodies, to remain always parallel, will carry us to a certain extent, in estimating the quantity of light contained in each of the two pencils, into which a beam is divided and subdivided: but it would soon appear, that, after a few modifications, this parallelism could no longer be supposed to be preserved: we should also find it impossible to assign the nature and extent of any forces, which might be capable of changing the former directions of the axes, and fixing them permanently in new ones. The distinction of a fixed, a moveable, and a partial polarisation, which has been imagined by Mr. Biot, must vanish altogether, upon considering that all the effects, which he attributes to the partial polarisation, are observable in experiments like those of Mr. Knox, in which there is confessedly no polarisation at all.

If we assume as a mathematical postulate, in the undulatory theory, without attempting to demonstrate its physical founda-

tion, that a transverse motion may be propagated in a direct line, we may derive from this assumption a tolerable illustration of the subdivision of polarised light by reflection in an oblique plane. Supposing polarisation to depend on a transverse motion in the given plane, when a ray completely polarised is subjected to simple reflection in a different plane, which is destitute of any polarising action, and may therefore be called a neutral reflection, the polar motion may be conceived to be reflected, as any other motion would be reflected at a perfectly smooth surface, the new plane of the motion being always the image of the former plane: and the effect of refraction will be nearly of a similar nature. But when the surface exhibits a new polarising influence, and the beams of light are divided by it into two portions, the intensity of each may be calculated, by supposing the polar motion to be resolved instead of being reflected, the simple velocities of the two portions being as the cosines of the angles formed by the new planes of motion with the old, and the energies, which are the true measure of the intensity, as the squares of the sines. We are thus insensibly led to confound the intensity of the supposed polar motion, with that of the reflected light itself: since it was observed by Malus, that the relative intensity of the two portions, into which light is divided under such circumstances, is indicated by the proportion of the squares of the cosine and sine of the inclination of the planes of polarisation. The imaginary transverse motion might also necessarily be alternate, partly from the nature of a continuous medium, and partly from the observed fact, that there is no distinction between the polarisations, produced by causes precisely opposed to each other, in the same plane.

Why light should or should not be reflected at certain surfaces, when it has been previously polarised, cannot, even with the greatest latitude of hypothesis, be very satisfactorily explained, but it is remarkable that the transmission is never wholly destroyed, or even weakened in any considerable proportion. We might, indeed, assign a reason for the occurrence of a partial reflection or a total transmission in the constitution of the surface concerned: since every abrupt change

of density must necessarily produce a partial reflection, while a gradual transition by insensible steps must transmit each impulse with undiminished energy, and without any reflection of finite intensity, as in the well known case of a collision, supposed to be performed with the interposition of an infinite number of balls of all possible intermediate magnitudes. If, therefore, we could find any modification of light, which could cause it to be transmitted from one medium to another in a more or less abrupt manner, we should thus be able to discover a cause of a variation of the intensity of the partial reflection: and this seems to be the nearest approach that we can at present make, to an explanation of the phenomenon, according to the undulatory theory.

Art. 6. (Sect. V.) The equal intensity of the colours of thin plates seen by reflection and by transmission, is a fact which could not have been expected from the immediate application of the law of interference, and which seems, therefore, at first sight, to militate against its general adoption. But this is only one of the many modifications of the law, which are the immediate consequences of its connexion with the undulatory theory; and it may be demonstrated, from the analogy of a series of elastic bodies, that no material difference in the intensity of the two kinds of colours ought to be expected in such circumstances. The intensity of a ray of light must always be considered as proportional to the energy or impetus of the elementary motions of the particles concerned, which varies as the square of the velocity, and not simply as the velocity itself: for if the velocity were made the measure of intensity, there would be an actual gain of joint intensity, whenever a ray is divided by partial reflection: since it follows from the laws of the motion of the centre of inertia, that when a smaller body strikes a larger, not the sum, but the difference of the separate momenta, will remain unchanged by the collision, while the sum of the energies remains constant in all circumstances; the square of a negative quantity being equal to that of the same quantity taken positively. Thus, supposing an elastic ball, 1, to strike another of which the mass is r, with the velocity 1, the velocity of the transmitted impulse will be

$\frac{2}{r+1}$, and that of the reflected, $\frac{2}{r+1} - 1 = -\frac{r-1}{r+1}$, the sum of the momenta in the opposite directions being $\frac{3r-1}{r+1}$, instead of 1, the original momentum; but the energies, expressed by the products of the masses into the squares of the velocities, will be $\frac{4r}{(r+1)^2}$, and $\left(\frac{r-1}{r+1}\right)^2$ respectively; and the sum of these is $\left(\frac{r+1}{r+1}\right) = 1$. Now, when an impulse arrives at the last of a series of larger particles, and is reflected in an inverted form, if we substitute $\frac{1}{r}$ for r, the energies will be in the proportion of $\frac{4}{r}$, and $\left(\frac{1}{r} - 1\right)^2$, or of $4r$ and $(1-r)^2$, which is the same as the former: so that, according to this analogy, the subdivision of the light at the second surface of a plate must be in the same proportion as at the first. We may call this proportion that of m^2 to n^2, $m^2 + n^2$ being equal to 1: we have then n^2 for the energy of the first partial reflection, m^2n^2 for the second; and m^2n^4 for the third: for the first transmission, into the substance, m^2; for the second, out of it, m^4; for the third, after an intermediate reflection, m^4n^2; and for the fourth after two reflections, m^4n^4; and the elementary velocities in either medium, compared among themselves, will be as the square roots of the respective energies. But it may be proved that, in all collisions of two moving bodies, each of the motions produces its effect on the velocities after impulse, independently of the other: so that the changes introduced, in consequence of the motion of one of the bodies concerned, are the same as it would have occasioned, if the other had been at rest; and consequently, if two undulations interfere in any manner, the joint velocities of the particles must always be expressed by the addition or subtraction of the separate velocities belonging to the respective undulations. When, therefore, the beam first partially reflected, of which the elementary velocity is expressed by n, interferes with the beam transmitted back, after reflection at the second surface, with the velocity m^2n, the joint velocity,

in the case of the perfect agreement of the motions, will be $n + m^2n$, and in case of their disagreement, $n' - m^2n$; the energies being $(n + m^2n)^2$ and $(n - m^2n)^2$; the difference, which is the true measure of the effect of the interference, being $4m^2n^2$, that is, four times the product of the respective velocities. But when the light simply transmitted at the second surface, with the velocity m^2, interferes with the light transmitted after two reflections, with the velocity m^2n^2, the quadruple product becomes $4m^4n^2$, only differing from the former in the ratio of m^2 to 1, which is that of the intensity of the light transmitted by the single surface to the intensity of the incident light, the difference being much too slight to be directly perceived by the eye: so that this result may be considered as agreeing perfectly with Mr. Arago's observation.

We may also obtain, from the analogy with the effects of collision, an illustration of the intensity of the partial reflection in different circumstances; although it is not easy to say what ought to be the precise value of r in the comparison. If we imagined the two mediums to differ only in density, while their elasticity remained equal, which is the simplest supposition, the density must be conceived to vary as the square of the velocity appropriate to the medium: but the value of r, thus determined, makes the partial reflection in general much too intense, and it becomes necessary to suppose it weakened by the intervention of a stratum of intermediate density, such as there is every reason to attribute to the surfaces of material substances in general, from the considerations stated in the article COHESION.* However this may be, we shall in general approach sufficiently near to a representation of the phenomenon, by taking the mass r in the simple proportion of the refractive density: thus, in the case of water, making $r = \frac{4}{3}$, we have for the energy of the first partial reflection $\left(\frac{r-1}{r+1}\right)^2 = \frac{1}{49} = .0204$, while the result of Bouguer's experiments is .018; and the agreement is as accurate as could have been expected, even if the whole calculation had

* No. XX. in this vol.

not been an imaginary structure. In the case of glass, the difference is somewhat greater; and it is natural to expect a greater loss of light from a want of perfect polish in the surface: for, taking $r = \frac{3}{2}$, we have $n^2 = .040$, and Bouguer found the reflection only .025. The surface of mercury reflected nearly .60; whence r should be about 8: whether the index of the refractive density can be so great as this, we have no precise mode of determining; but there seems to be something in the nature of metallic reflection, not wholly dependent on the density: thus it may be observed that potassium has a very brilliant appearance, though its specific gravity is very low; at the same time, its great combustibility might give it a much higher rank among refractive substances than could otherwise have been expected from its actual density.

Art. 7. (Sect. XIII.) Although the ingenuity of man has not yet been able to devise anything like a satisfactory reason for the reflection of a polarised ray in one case, and its transmission in another: yet several attempts have been made, with various success, to reconcile the different hypotheses of light with the other phenomena of oblique refraction. The illustrious Mr. Laplace has undertaken to deduce the laws of this refraction according to the projectile system, from the general doctrines of motion; and he has sufficiently demonstrated that the path followed by the light is always such, as to agree with the principle of the least action, supposing the law of the velocities previously established; or in other words, that the sum of the products of the spaces described, into the respective velocities, is always the least possible. To this demonstration it has been objected, that notwithstanding the complication of its steps, it is in fact nothing more than the simple translation of the fundamental law of Huyghens into another language; for it is assumed in this theory, upon obvious and intelligible grounds, that the path of light must always be such, that the time may be equal with respect to two neighbouring collateral parts of the undulation; which is the well known condition of a minimum of the whole time employed; and the time being always expressed by the space

divided by the velocity, if we suppose the proportions of the velocities to be inverted, as in the two theories respecting light, the expression of the time in the one will be identical with that of the action in the other: consequently the conditions of the propagation of light, in the Huyghenian doctrine, must always imply the observation of the law of least action in the opposite hypothesis: and this general proposition Mr. Laplace has taken great pains to prove, with respect to a particular instance, in which the Huyghenian calculation had been found, notwithstanding Newton's doubts, to agree perfectly with the phenomena. It has also been observed, that the law of the least action is wholly inadmissible as a fundamental principle of motion: that it is completely unphilosophical to multiply unnecessarily the number of postulates or elementary laws: and that although in many cases the principle may be capable of being established as a derivative proposition, yet, in order to demonstrate it, we must assume that the velocity must be the same in all directions, between the same parallel or concentric surfaces, or, at any rate, must limit our reasoning by conditions incompatible with the nature of the actions, to be considered as the foundation of the laws of extraordinary refraction.

In this single point the undulatory theory has every possible advantage over its rival. For the difference of the velocities in different directions no force has been assigned as a cause, in the projectile system, at all more general than the individual directions of the rays with respect to the axis. But upon the hypothesis of undulations it has been demonstrated, without any gratuitous supposition, that every lamellar or fibrous substance must transmit every diverging impulse in the form of a spheroidal surface, supposing only the elasticity to act more powerfully in one direction than another, as it naturally must do in such circumstances.* And when we consider the experiments of Dr. Brewster and Dr. Seebeck, which show that any compression or extension whatever, acting in a given direction on any transparent solid, is capable of occasioning those appearances of colour, which prove the transmission

* Supra, No. XII.

of light by an ordinary and an extraordinary refraction,* we cannot help imagining that the cause of the elliptic refraction must be of a nature more simple than is consistent with the existence of a multiplicity of attractions, and repulsions, and polarising powers, acting in all manner of directions, with every possible variation of intensity, independently of any assignable variation of the circumstances of the light affected.

There is, however, another mode of considering the mechanism of elliptic refraction, which is somewhat less simple and elementary, but which affords us a further analogy to the phenomena of polarisation by reflection and transmission. If we suppose a doubling crystal to consist of a large number of very thin plates, united by a medium differing but little from them in refractive density, this pile will completely polarise the transmitted light, as Malus and Dr. Brewster have shown, in a plane perpendicular to that of the incidence, even when the inclination of the surfaces is widely different from that which produces the most complete polarisation at once; and at the same time a part of the light, partially reflected by each plate, will be again reflected by the neighbouring surfaces into its original direction; nor will it be difficult to imagine that the quantity of light thus twice reflected, may, from some unknown cause, be rendered ultimately equal to that of the light simply transmitted, which, according to the laws of polarisation, will never exceed half of the original beam. Now the lengths of the paths of these two portions will not only be different, but the difference will vary according to the direction: for while the light simply transmitted proceeds to describe its path with the uniform velocity belonging to the mean of the two mediums, combined always in a constant proportion, the light twice reflected by each plate will be retarded most of all in the direction perpendicular to the plates, as in the case of the colours of common thin plates; and in oblique directions, the number of plates which it has to pass through in a given space, being as the cosine of the angle of incidence, and the retardation in each plate being also as the same cosine, neglecting the difference between the angles of incidence and refraction, which is supposed

* See No. XVIII. in this volume.

to be inconsiderable, the whole retardation will be as the square of the cosine of the inclination to the axis, which is the well-known proportion of the difference of the diameters of a circle, and of an ellipsis approaching near it. We thus obtain a general idea of the combination of two effects, which do not appear to be related in any other point of view, a regular oblique refraction, and a distinct polarisation; further than this, the comparison is by no means completely satisfactory: and the great difficulty of all, which is to assign a sufficient reason for the reflection or non-reflection of a polarised ray, will probably long remain, to mortify the vanity of an ambitious philosophy, completely unresolved by any theory.

No. XVI.

REMARKS ON THE MEASUREMENT OF MINUTE PARTICLES, ESPECIALLY THOSE OF THE

BLOOD AND OF PUS.

From Dr. Young's Introduction to Medical Literature, p. 545.

1. *On the Form and Magnitude of the Particles of the Blood.*

THE form and magnitude of the coloured particles of the blood is a subject not only interesting and important in itself, but is also capable of assisting, by means of comparative observations, in the determination of the magnitude of the capillary arteries, and the investigation of the resistance which they exhibit; it may also be of advantage to obtain some tests capable of ascertaining, whether these particles undergo any change in diseases of various kinds, and what is their relation to the globules of pus, and of other animal fluids: hitherto the measures of the particles of blood, which have been considered by various authors as the most accurate, have differed no less than in the ratio of 2 to 5; and there is an equal degree of uncertainty respecting their form, some admitting the truth of Mr. Hewson's opinions, and a greater number rejecting them without any satisfactory evidence. In such examinations, it is only necessary to employ a full and unlimited light, in order to obtain a very distinct outline of what appears manifestly to be a very simple substance, and we thus seem to have the clear evidence of the senses against Mr. Hewson: but we must remember, that where the substances to be examined are perfectly transparent, it is only in a confined and diversified light that we can gain a correct idea of their structure. The eye is best prepared for the investigation, by beginning with the blood of a skate, of which the particles are so conspicuous, and of so unequivocal a form, as to set aside at once the idea of a simple homogeneous sub-

stance. They are oval and depressed, like an almond, but less pointed, and a little flatter; each of them contains a round nucleus, which is wholly independent in its appearance of the figure of the whole disc, being sometimes a little irregular in its form; seldom deviating from its central situation, but often remaining distinctly visible while the oval part is scarcely perceptible; and as the portion of blood dries away, becoming evidently prominent above the thinner portion. This nucleus is about the size of a whole particle of the human blood, the whole oval being about twice as wide, and not quite three times as long; the nucleus is very transparent, and forms a distinct image of any large object which intercepts a part of the light by which it is seen, but exhibits no inequalities of light and shade, that could lead to any mistake respecting its form. But if we place some particles of human blood under similar circumstances, near the confine of light and shade, although they are little, if at all, less transparent, we immediately see an annular shade on the disc, which is most marked on the side of the centre on which the marginal part appears the brightest, and consequently indicates a depression in the centre, which Delatorre mistook for a perforation. It is most observable when the drop is drying away, so that the particles rest on the glass: and when a smaller particle is viewed, it has merely a dark central spot, without any lighter central space. Nor have the particles ever appeared to me "as flat as a guinea," although their axis is sometimes not more than one third or one fourth of their greatest diameter; if they were much thinner than this, their diameter would be more diminished than it is when they become spherical, by the effect of an aqueous fluid; while this form corresponds to a diminution to about $\frac{2}{3}$ of the original diameter. They may indeed possibly absorb a part of the surrounding moisture in the change; but they do not seem to have their dimensions much affected by the fluid in which they are suspended, since they may easily be spread thin on glass, and dried, without much change of their magnitude, at least in the direction of the surface to which they adhere: and they remain distinct as long as the access of moist air is completely excluded. When they have been kept for some time

in water, and a little solution of salt is added, their form and structure, as Mr. Hewson has observed, are more easily examined, and appear to resemble those of a soft substance with a denser nucleus, not altogether unlike the crystalline lens together with the vitreous humour, as seen from behind: but with respect to a central particle detached within a vesicle, "like a pea in a bladder," I cannot doubt that Mr. Hewson was completely mistaken. I have never observed a prominence in the outline of the particles of the human blood: and on the other hand I am not perfectly confident that the apparent depression, which is exhibited in some lights, may not depend on some internal variation of the refractive density of the particle. It has commonly been asserted, that these coloured particles are readily soluble in water; but this opinion appears to be completely erroneous, and to depend partly on their passing readily through filtering paper, a circumstance indeed already observed by Berzelius, (Djurk. II. p. iii.) and partly on the extraction of a great part of their colouring matter, together with which they lose much of their specific gravity, so that instead of subsiding, they are generally suspended in the fluid; their presence may however still be detected by a careful examination, and they seem in this state to have recovered in some measure their original form, which they had lost when first immersed in the water. When the water is sufficiently diluted, about three-fourths as much rectified spirits may be added to it without destroying the appearance; but after a few months it becomes indistinct, although neither in this case nor in that of complete putrefaction do the globules appear to become constituent parts of a homogeneous fluid. The existence of solid particles, in fluids which at first sight appear transparent, is the most easily detected by looking through them at a small luminous object, either directly or by reflection, as, for example, at the image of a candle seen at the edge of a portion of the fluid, held in a teaspoon; in this case, wherever there are small particles in suspension, for instance, in milk diluted with water, they will produce a minutely tremulous or sparkling appearance, which is rendered still more distinct by the assistance of a lens, and which depends on the diversified interception of the

light, while the particles are carried over each other by the internal motion of the fluid. This test is applicable to all cases of minute particles held in suspension; where, however, the greater number of the particles are nearly equal in dimensions, the luminous object viewed through them exhibits a much more striking appearance, for it is surrounded by rings of colours, somewhat resembling those of the rainbow, but differently arranged, and often beautifully brilliant. The blood, a little diluted, always exhibits them in great perfection, and they afford a very accurate criterion for the distinction between pus and mucus: mucus, containing no globules, affords no colours, while those which are exhibited by pus exactly resemble the appearance produced by the blood, the rings being usually of the same dimensions: whence it follows that the globules are also of the same size, for the dimensions of the rings vary with those of the particles which produce them: and there can be little doubt, from this circumstance, that the globules found in pus are the identical globules of the blood, although probably somewhat altered in the process of suppuration. A minute quantity of the fluid to be examined in this manner may be put between two small pieces of plate glass, and if we hold the glass close to the eye, and look through it at a distant candle, with a dark object behind it, the appearance, if the globules are present, will be so conspicuous as to leave no doubt respecting their existence.

2. *Description of the Eriometer.*

The rings of colours, which are here employed to discover the existence of a number of equal particles, may also be employed for measuring the comparative and the real dimensions of these particles, or of any pulverised or fibrous substances, which are sufficiently uniform in their diameters. Immediately about the luminous object, we see a light area, terminating in a reddish dark margin, then a ring of bluish green, and without it a ring of red: and the alternations of green and red are often repeated several times, where the particles or fibres are sufficiently uniform. I observed some years ago that these rings were the larger as the particles or fibres affording them were

smaller, but that they were always of the same magnitude for the same particles. It is therefore only necessary to measure the angular magnitude of these rings, or of any one of them, in order to identify the size of the particles which afford them; and having once established a scale, from an examination of a sufficient number of substances of known dimensions, we may thus determine the actual magnitude of any other substances which exhibit the colours. The limit between the first green ring, and the red which surrounds it, affords the best standard of comparison, and its angular distance may be identified, by projecting the rings on a dark surface, pierced with a circle of very minute holes, which is made to coincide with the limit, by properly adjusting the distance of the dark substance, and then this distance, measured in semidiameters of the circle of points, gives the corresponding number of the comparative scale. Such an instrument I have called an Eriometer, from its utility in measuring the fibres of wool, and I have given directions for making it, to Mr. Fidler in Foley Street. The luminous point is afforded by a perforation of a brass plate, which is surrounded by the circle of minute holes; the substance to be examined is fixed on some wires, which are carried by a slider, the plate being held before an Argand lamp, or before two or three candles placed in a line; the slider is drawn out to such a distance as to exhibit the required coincidence, and the index then shows the number representing the magnitude of the substance examined. The instrument may be rendered more portable, though somewhat less accurate, by merely making the perforations in a blackened card, furnished with a graduated piece of tape. An eye not shortsighted will generally require the assistance of a lens, when the instrument is made of the most convenient dimensions, which I have found to be such as to have two circles of points, one at $\frac{1}{8}$ and the other $\frac{1}{4}$ of an inch in semidiameter, with their corresponding scales. The central perforations are about $\frac{1}{30}$ and $\frac{1}{50}$ of an inch in diameter; the points 8 or 10 only in each circle, and as minute as possible. The light of the sun might also be employed, by fixing the circle of points at the end of the tube of a telescope: but it rather adds glare than distinctness to the colours: nor have I

been able to gain anything by looking through coloured glasses, or by using lights of different qualities. Where the object consists of fibres which can be arranged in parallel directions, a fine slit in the plate or card affords brighter colours than a simple perforation, and the points must in this case be arranged in lines parallel to the slit; but if care is not taken to stretch the fibres sufficiently, the employment of the slit in this manner will make them appear coarser than they really are. The colours will still appear, even if there be a considerable difference in the dimensions of the fibres or particles, but they will be so much the less distinct as the difference is greater. In this case the measure indicated will be intermediate between the extreme dimensions; although most commonly it will be somewhat below the true mean, the colours exhibited by the finer fibres prevailing in some degree over the rest. The latitude, however, which the Eriometer affords in the regularity of the substances measured by it, and its collecting into one result the effect of many thousands of particles, or of an endless variety of small differences in the diameters of fibres, give it an unquestionable preference over every kind of micrometer which measures a single interval only at once, with respect to all applications to agriculture or manufactures; for in reality there is not a single fibre of wool among the millions which constitute a fleece, that preserves a uniform diameter throughout its length, and the difference is still greater between the fibres which grow on different parts of the animal; so that to take a single measurement, or even any practicable number of measurements, by the most accurate micrometer, in the usual acceptation of the term, for a criterion of the quality of a fleece, can tend only to the propagation of error or conjecture in the semblance of the minutest accuracy. Even with the Eriometer, the difficulty of obtaining a fair average of the quality of a sample of wool is extremely great; it is absolutely necessary to preserve the fibres as much as possible in their natural relative situation, and to examine them near the middle of their length; the ends next the skin are almost always considerably finer, and the outer ends generally coarser, than the rest; but this difference is greater in some kinds of sheep than in others, and as far as I

have observed, it is less in the Merinos and their crosses than in other sheep: there is also far less difference in the different parts of the same fleece in these breeds than in others; still however this difference is very observable, although it is probable that some part of the sheep might be found, which in all cases might fairly be considered as affording nearly the average of the whole fleece; and I imagine that the part of the back about the loins is the most likely to be possessed of this property; so that the middle of the fibres of this part of the fleece might be assumed, in the finer kinds of wool, as affording a fair measure for the whole.

3. *Scale of Eriometer.*

The theory, which suggested to me the construction of the eriometer, requires some corrections in its immediate application, which depend on circumstances not completely understood: at present therefore I shall only employ, for the determination of the true value of the numbers of its scale, an experimental comparison of its indications with some microscopical measurements, which Dr. Wollaston has been so good as to perform for me, with an admirably accurate micrometer of his own invention.

The dust or seed of the lycoperdon bovista he finds to be $\frac{1}{8500}$ of an inch in diameter: this substance gives very distinctly 3.5 on the scale of the Eriometer; and $3.5 \times 8500 = 29750$. The globules of the blood measured $\frac{1}{4900}$; and immediately afterwards, when examined in the same state by the Eriometer, indicated about $6\frac{1}{2}$; and $6.5 \times 4900 = 31850$. A wire of platina, obtained by a very ingenious method, peculiar to Dr. Wollaston, measured $\frac{1}{3200}$; and when coiled up gave n. 9 of the Eriometer; and $9 \times 3200 = 28800$. The mean of a considerable number of comparative observations on fibres of wool, between n. 20 and 30, afforded also 28800 for a product.

A mean of these experiments gives very nearly $\frac{1}{30000}$ for the unit of the scale of the Eriometer. Some former investigations had led me to attribute to this unit a value somewhat smaller, especially for the lowest numbers; and I had obtained a formula, and made a table, for ascertaining the true dimensions of

any substance measured by the instrument, according to the result of these investigations; but since my later experiments seem to have superseded the mode of calculation which I had adopted, I think it unnecessary to insert the table.

Having sufficiently ascertained the true value of the indications of the eriometrical scale, I shall now enumerate the measurements of the principal substances which I have examined with the instrument.

4. *Substances measured by the Eriometer.*

Milk, diluted, very indistinct, about	3
Dust of lycoperdon bovista, very distinct	3½
Bullock's blood, from beef	4½
Smut of barley, called male ear	6⅓
Blood of a mouse	6½
Human blood diluted with water, 5; after standing some days 6, or	7
Blood recently diluted with serum only	8
Pus	7½
Silk, very irregular, about	12
Beaver wool, very even, (jointed)	13
Angola wool, about	14
Vigonia wool	15
Siberian hare's wool, Scotch hare's wool, Foreign coney wool, Yellow rabbit's wool, about	15½
Mole's fur, about	16
Skate's blood, very indistinct, about	16
American rabbit's wool, British coney wool, about	16½
Buffalo's wool (B)	18
Wool of the ovis montana (D)	18
Finest seal wool, mixed, about	18½
Shawl wool 18 or	19
Goat's wool	19
Cotton, very unequal, about	19
Peruvian wool, mixed, the finest locks	20
A small lock of Welsh wool (B)	20
Saxon wool, a few fibres 17, some 23, chiefly	22
An Escurial ram, at Ld. Somerville's show, 23 to	24

Mr. Western's South Down, some specimens	24½
Lioneza wool, 24 to 29, generally	25
Paular wool, 24 to 29, generally	25½
Alpacca wool, about	26
Farina of laurustinus	26
Ryeland Merino wool, Mr. Henty	27
Merino South Down wool, Mr. Henty	28
Seed of lycopodium, beautifully distinct	32
South Down ewe, Mr. W. B.	39
Coarse wool, Sussex	46
Coarse wool, from some worsted	60

It would not be difficult to obtain from these measures a tolerable approximation to the value of wool at its usual prices. If we square the number, and subtract 325, the remainder will be about the number of pounds that are worth 100 guineas. Thus, for good Lioneza, n. 25, $25 \times 25 - 325 = 300$, giving 7*s.* a pound; for moderate South Down, n. 35, $35 \times 35 - 300 = 900$, or 2*s.* 4*d.* a pound: which is probably about the proportional value, though both the proportional and the real values must fluctuate according to the demand of the manufacturer.

5. *Microscopical fallacies.*

I shall here take the liberty of inserting some remarks, which I cannot attempt at present to render intelligible to any, who have not entered into the minutest refinements of physical optics: to such as are unacquainted with the latest investigations, I fear they must appear involved in a degree of obscurity almost enigmatical.

When a small object is viewed in a microscope, especially if the light is admitted by a limited aperture, it will often appear to be surrounded by some lines of light and shade, or of colours, which might be supposed to depend on its magnitude, in the same way that the eriometrical colours are derived from the magnitude of the objects examined. In reality, however, their existence and their dimensions depend on the aperture of the microscope, and not on the magnitude of the particles in its focus. To prove that this aperture may produce such an effect,

hold any object, for instance, the finger or the nail, so as to intercept all the light of a candle, except a narrow line, and this line will seem to project other lines parallel to it into the adjoining shade. Now these lines depend on the interposed object on one side, and on the margin of the pupil on the other: for if we take an object a little narrower than the pupil, we may see them on both sides of it; and causing the pupil to contract by throwing more light on the opposite eye, they will expand, as the space, through which they are admitted, is diminished by the contraction. We may also very distinctly observe, if we look in this manner at a narrow line of light instead of a candle, that the dispersive powers of the eye manifestly convert its image on the retina into a spectrum of red, green, and blue light; sufficiently confuting the conjectural hypothesis of the achromatic property of its refractive substances. If again we substitute a minute hole or slit in a card for the interposed object, the sides of this aperture will now determine the magnitude of the fringes which are seen at the edge of the candle, and their dimensions will be no longer variable, whatever may be the state of the pupil. But the candle must in this case either be placed at a distance or be partly concealed from the eye, unless one edge of the aperture project so far beyond the other, as to limit its visible extent. Now the substance, in which the lens of a microscope is contained, presents a small aperture capable of exhibiting effects of this kind, which however can only be expected to appear when the light is peculiarly circumstanced. The aperture of the highest magnifier that I have employed is $\frac{1}{50}$ of an inch, which answers to about n. 330 of the scale of the Eriometer, and would consequently exhibit a bright ring at $\frac{1}{330}$ of the distance of a minute object viewed through it, while the darkest part within this ring would be at about $\frac{2}{3}$ of that distance: and the focal distance of the lens being about $\frac{1}{45}$ of an inch, the diameter of the apparent dark circle would be $\frac{1}{1140}$ of an inch, and that of the bright one $\frac{1}{760}$; and the dimensions would be nearly the same if any other small lens were employed, with an aperture half as great as its focal distance; so that the constancy of such an appearance, notwithstanding a change of

magnifiers, might increase the probability of error. It is obvious that a shade of this kind, surrounding the central parts of a globule, if they happened to be much brighter than the rest, might give rise to a mistaken idea of inequalities in its form or structure; and it is possible that when a particle is darker than the surrounding medium, some parts of its surface may have lines of a similar nature projected on them in an inverse order. The particles of the blood are about $\frac{1}{3000}$ of an inch in diameter, varying from $\frac{1}{6000}$ to $\frac{1}{4000}$; and it is extremely possible that an object of these dimensions may exhibit a light point near its centre, which may be surrounded by a dark and then by a light annular shade within the limits of its disc. There are also several other sources of error in different lights, and in a focus more or less imperfectly adjusted; it is however sufficiently evident that no fallacy of this kind can have given rise to all the appearances, which have been already described, as observable in the particles of the human blood, and still less to those which are observable in the blood of some other animals.

6. *Changeable Colours.*

In examining some of the dust of the lycoperdon, I had put it with a drop of water on a glass, when I observed a purple tinge in the water, which I thought at first was a stain extracted from the powder; but the water viewed separately was perfectly transparent, and the light transmitted directly through the water, when the globules were present, was of a yellowish green. After some consideration, I conjectured that this appearance of colour must be analogous to that of the mixed plates which I had formerly observed, depending on the difference of refractive density of the water and the globules, (Nat. Phil. I. 470. Pl. 30. F. 430,) and by substituting fluids of different densities for water, I had the pleasure of finding my conjecture confirmed; for when the water was saturated with salt, the yellow green became nearly blue, and the purple redder or browner; and when olive oil was employed, the light directly transmitted was purple, and the oblique light greenish; in balsam of Tolu again, this purple became red, and the indirect

light afforded a faint blue. In air too, I found that the powder appeared of a bright blue green by direct light, and of a purplish hue with a light a little oblique; but when the obliquity became a little greater, the tint changed to a brownish yellow green, which continued afterwards unchanged; this alteration may perhaps be derived from the admixture of a portion of light coming round the particles by a more circuitous route. By comparing the opposite effects of water and olive oil, of the refractive densities 1.336 and 1.379, the refractive density of the particles themselves may be calculated to be 1.62, or somewhat less.

Grey beaver wool seems of a purplish hue in direct, and greenish in oblique light, both in air and in olive oil; its grey colour seems to be derived from a mixture of these tints; in olive oil, the rings of colours which it affords are considerably altered in their appearance, the reds becoming every where very faint. Lead precipitated from its acetate, or silver from its nitrate, by common water, affords a reddish direct and a bluish indirect light, and the same seems to be true of smoke, and of other bodies consisting of very minute particles; but when the indirect light is very powerful, smoke sometimes appears reddish in it, as might be expected from a collection of very small opaque instead of transparent particles.

Mr. Delaval has observed that an infusion of sap green appears of a bright red by transmitted light, and the case seems perfectly analogous to that of the dust of the lycoperdon; the green becoming somewhat yellower, when the gum, with which the colouring particles are mixed, is diluted with water. But this is not the universal cause of a difference of colours exhibited by pigments in different lights; the carthamus, or pink dye commonly sold for domestic use, affords an unequivocal instance of a substance exhibiting colours analogous to those of thin plates, which have been adduced by Newton, in illustration of the colours of natural bodies; the reflected light being undeniably of a yellow green, while the transmitted light is of a bright pink colour. Here the light regularly reflected from the surface only, especially when dry, gives the colour opposite to that of the transmitted light; all the light passing through the fluid, even

indirectly, giving a pink colour. But the infusion of the lignum nephriticum seems to hold a middle place between this substance and those which have been mentioned before; the dry extract is of a brownish yellow only; an infusion, not too strong, gives the same colour, verging to orange, by direct transmitted light, and a bright blue by light reflected, or obliquely dispersed within the infusion, or at its surface. The solution of the carthamus affords no green reflection from its surface, and varies in its hue, in different lights, only from crimson to scarlet. The tinging particles of the lignum nephriticum, like those of the precipitated lead and silver, are probably extremely minute, since the colour is but little changed by changing the density of the fluid. It often happens that a blue colour, precisely like that of this infusion, is reflected by green glass bottles, which, when seen by transmitted light, exhibit only a reddish brown colour. The inner bark of the ash is also said to have a property similar to that of the lignum nephriticum. (Murr. app. med.) The particles of the blood do not derive their colour from any of the causes which have been mentioned, since it may be extracted from them in a clear solution.

When I attempted to explain the colours of mixed plates, which I had produced by partially moistening two lenses very slightly convex, I observed that the reflection of the light from the internal surface of a denser medium must be supposed to invert its properties with respect to the production of colours by interference, as is naturally to be supposed on the principles of the undulatory theory. But when the obliquity is so considerable, it is not very easy to assign a reason for this inversion; and the experiments, which I have now mentioned, make it necessary to assume a law, which I cannot explain, that every very oblique reflection inverts the properties of light with respect to interference. This conclusion confirms the assertion of Newton, that a dark space, bordered by light, will appear in the centre of a portion of light transmitted between the edges of two knives placed very near each other, and the opinion of Mr. Jordan, that the augmentation of a shadow by diffraction is to be considered as the first dark space belonging to the coloured fringes. I had obtained a different result in an ex-

periment similar to Newton's, because I was not aware of the necessity of employing very sharp edges; for when the edges are blunt, the light is reflected from the one to the other in such a manner, as wholly to destroy the appearance of a central dark space; but in any case this source of error may be avoided, by causing one of the edges to advance a very little before the plane of the other, so that half of the fringes may disappear. It is however necessary to suppose this inversion confined to cases of extremely oblique reflection, for when the deviation of the light from a rectilinear path becomes a little more considerable, its effects are no longer perceptible; the second and third fringes scarcely ever requiring any material corrections of the calculations from which it is excluded. The same inversion must also be attributed to the light bent by diffraction round the remoter side of a fibre: for this light always co-operates in the first instance with that which is reflected from the nearer side. The extent of the central white light is indeed so great, that all the coloured appearances may almost be considered as beginning at such a distance, that the first dark space is exactly where the simple calculation would lead us to expect the white; since the value of the unit of the Eriometer ought to be, according to this calculation, about $\frac{1}{44000}$ of an inch, instead of $\frac{1}{30000}$; and indeed this value agrees very accurately with experiment, where the two portions of light concerned are exactly in similar circumstances; as may be observed in some of the parallel lines drawn on glass in Mr. Coventry's micrometers, probably where they happen to be single, for in general they are double, and exhibit colours corresponding to an interval much smaller than their regular distance: but in some parts we may observe colours exactly corresponding to their distance, for instance, to $\frac{1}{500}$ of an inch, according to the simple principle of considering each unit as equal to about the 43000th of an inch. Hence it seems that the necessity of a correction depends on the different state of the lights reflected from one side of a fibre, and diffracted round its opposite side, and that when they proceed in a similar manner from two neighbouring parallel lines, the necessity no longer exists. What may be the cause of this irregularity, will perhaps be understood when we understand the

cause of the singular phenomena of oblique reflection discovered by Mr. Malus, and we have no reason to expect to understand it before.

7. *Glories.*

I have had an opportunity of ascertaining, that the clouds which exhibit the white and coloured circles, sometimes denominated glories, are certainly not composed of icy particles; and I have succeeded in deducing an explanation of these phenomena from the same laws, which are capable of being applied to so many other cases of physical optics. In the theory of supernumerary rainbows, (Nat. Phil. I. 471. Pl. 30. Fig. 451. *supra*, p. 185), I have observed that the breadth of each bow must be the greater as the drops which afford it are smaller; and by considering the coloured figure, in which their production is analysed, it will be obvious, that if we suppose the coloured stripes extremely broad, they will coincide in such a manner in one part as to form a white bow: the red, which projects beyond the rest, being always broadest, so that if all the stripes be supposed to expand, while they preserve their comparative magnitude, the middle of the red may coincide with the middle of the blue; and it will appear on calculation that a white bow will be formed, a few degrees within the usual place of the coloured bow, when the drops are about $\frac{1}{3000}$ or $\frac{1}{4000}$ of an inch in diameter. It is remarkable that in such cases the original rainbow is altogether wanting, and probably for a similar reason, we scarcely ever see a rainbow in a cloud which does not consist of drops so large as to be actually falling, although I have once seen such a rainbow ending abruptly at the bottom of a cloud: it may be conjectured that the edge of the light is in such cases so much weakened by diffraction, that it is too faint to exhibit the effects occasioned by a larger drop. Dr. Smith has made a remark somewhat similar, (Opt. r. 501.) which, if not completely satisfactory upon the principles which have been mentioned, is certainly altogether unintelligible upon his own.

The coloured circles, immediately surrounding the shadows of the observers, may be deduced from the effect of the same

minute particles of water, upon the light which has been four, and perhaps five, times reflected within the drops, which may, after transmission, coincide in direction with another portion, passing on the opposite side of the centre; and the drops about $\frac{1}{3000}$ or $\frac{1}{4000}$ of an inch in diameter would in this manner produce a faint corona, of such magnitude, that the limit of green and red, employed in the use of the eriometer, should be at the distance of about five degrees from the centre of the shadow; which, as nearly as I could estimate it, was its real distance in the appearance that I observed.

No. XVII.

SELECTIONS FROM CORRESPONDENCE RELATING TO

OPTICAL SUBJECTS.

1.—*From* Dr., *now* Sir DAVID BREWSTER *to* Dr. YOUNG.

DEAR SIR, Edinburgh, 13, Hope-street, July 28, 1815.

I AVAIL myself of an opportunity which has just offered itself, of sending you an account of an optical discovery in which I have no doubt you will be much interested, as it appears to give very great support to your opinion; that the colours produced by the action of crystallised bodies upon polarised light are referable to your theory of periodical colours.

Having some time ago discovered that all the metals acted upon polarised light, like crystallised laminæ, a single reflexion polarising the same colour as a plate of sulphate of lime of a given thickness, it occurred to me that a similar effect might be produced by *total reflexion* from the second surfaces of all transparent bodies. I had great difficulty, however, in making this experiment, as almost every glass prism is more or less crystallised; but by enclosing water in hollow prisms of glass that exercised no action upon light, I ascertained that one total reflexion from the second surfaces of transparent bodies polarises a tint of the first order of Newton's table in exactly the same manner as a plate of sulphate of lime. Two or more total reflexions in the same plane polarise colours higher up the table like two or more films of sulphate of lime, having their axes coincident; while two total reflexions at the same angle and in planes of right angles to each other, counteract each other like two equally thick plates of sulphate of lime, having their axes crossed at right angles.

There is here no action of a doubly refracting force: the totally reflected pencil consists of two oppositely polarised portions, one of which, namely, the one which is polarised in a plane at right angles to the plane of reflexion, has advanced farther through the transparent body, and therefore suffers reflexion, or changes its direction later than the other pencil. Hence colours ought to be produced.

I have no doubt that the preceding phenomena will be considered as hostile to Biot's theory of oscillations, and as highly favourable to your theory of recurrent colours.

I have lately found that muriate of soda and fluor spar, which Malus, Biot, and Arago have always regarded as possessing simple refraction, do possess the properties of all doubly refracting crystals: at a thickness of between 1 and 3 inches they polarise the blue of the first order of Newton's table, and exhibit regular optical axes. You would oblige me by not mentioning any of the preceding properties, as I have not yet finished the investigation of them.

I have the honour to be, dear Sir,

Ever most truly yours,

D. BREWSTER.

2.—Dr. YOUNG *to* Sir D. BREWSTER.

DEAR SIR, Worthing, 13th September, 1815.

I FEAR you have thought me remiss in not returning an earlier answer to your kind communication: but I have been waiting until I could find leisure to enter a little more fully into the subject, than I had it in my power to do when I received your letter. I am glad that a circumstance has occurred, which has led you to think with some attention of my theory of the interference of light, because I am quite certain that it must become particularly interesting to you, who have observed so many phenomena which appear to be most intimately related to it, and which admit of a striking illustration by its means. I cannot, however, see that it has much connexion with the facts which you state respecting internal reflection: if my memory does not deceive me, some former optician, perhaps

Bouguer, has observed a little difference of colour in light totally reflected, but I have no note of the observation by me: nor can I attempt to form anything like an opinion of the nature of this appearance of colour, or of that which you mention as produced by the reflection of metals, not *tarnished*, without a more particular detail of the experiment.

With respect to my own fundamental hypotheses respecting the nature of light, I become less and less fond of dwelling on them, as I learn more and more facts like those which Mr. Malus discovered: because, although they may not be incompatible with these facts, they certainly give us no assistance in explaining them. But this observation does not extend to my laws of interference, as explanatory of the phenomena of periodical colours: since almost every new case of the production of colours, that has been lately discovered, ranges itself as a simple consequence of these laws, and is as regularly deducible from them by calculation, as the motions of the planets are deducible from the laws of gravitation; nor are the laws any more invalidated by disproving the Huyghenian system of undulations, than the Huyghenian law for the determination of oblique refraction was rendered less accurate by Newton's supposed confutation of the same hypothesis, or than the laws of gravitation would be superseded if we allowed the validity of poor Professor Vince's arguments against the Newtonian ether.* You were apparently impressed with a very high respect for the generalisation of the phenomena of the colours of plates of doubly refracting crystals, which Mr. Biot had obtained, although his rules are derived from direct observation only, and merely enable us to ascertain the colour of a given plate of some few given substances, viewed in a given direction.

* Professor Vince's 'Memoir on the Cause of Gravitation' was presented to the Royal Society, and selected, in the first instance, as the Bakerian lecture, though it was subsequently, upon a more deliberate examination, rejected as unfit for publication in the Transactions: it was afterwards published as a pamphlet, with a preface, in which the treatment it had experienced was in some degree attributed to the prejudice of the president, Sir J. Banks, against the party in the Society, headed by Dr. Hutton, to which the author had attached himself. This pamphlet was made the subject of some very severe and somewhat unjust critiques by Dr. Young, under the signature 'Dytiscus,' in Nicholson's Journal for 1808: neither the character nor the works of Professor Vince were such as to justify the contemptuous reference to him which is made in the text.—*Note by the Editor.*

But you do not seem to have been at all struck by my deductions of the same conclusions, or of conclusions agreeing equally well with the experiments from the general principles of the laws of interference only, combined with the known refractive powers of the substances concerned: and I must confess that I was not a little astonished to find that the deduction seemed to make no impression whatever on Biot, when I sent it him: for in a very civilly intended answer, he merely referred me to his own book, which I have not read, and which I believe I shall not read, because I find that he begins with suppositions respecting the motions of the particles perfectly incompatible with the general laws of mechanics. I can only suppose that I had not made myself sufficiently intelligible to him: otherwise the coincidence of my determination of the actual thickness affording a given colour and of the effects of obliquity and of inclination in all directions with the diversified facts which he had ascertained, must have been too striking to have escaped him. Nothing can be simpler than the principle on which my calculation proceeded, that there must be two sets of rays in each direction, one transmitted by ordinary, the other by extraordinary refraction, and that the difference of the velocities of these rays must cause the phenomena of interference in the same manner as if they had actually arrived by paths of different lengths: and nothing can be more intricate than the results, if examined without a clue.

Had the consequences of this law been present to your mind, you could scarcely have avoided observing how completely it explained the phenomena that you have described in the printed papers which you have been so good as to send me. The rays, twice or thrice reflected, are transmitted through the two glasses at an obliquity a little different; and this difference is sufficient to produce the same phenomena as the passage of the rays through a single plate of which the thickness corresponds to the difference: just as in Mr. Knox's experiments the effects are precisely the same as those of a plate of which the thickness is equal to the difference of the thicknesses of the two plates of glass or of air concerned: a phenomenon which I had long ago explained in my Lectures, in a remark on an observation of

Mr. Nicholson: you will find it under the head Colours from Interference, in the second volume. You have observed that the Newtonian doctrine of fits of easy refraction and reflection sufficiently explains the phenomena of thin and of thick plates; but you surely cannot have considered this subject with attention, otherwise you would have been aware that when the path of the ray is lengthened by its obliquity with respect to the surfaces, the number of fits ought naturally to be increased, while in reality it is diminished: and I must request you to tell me how you explain the white circle from which the colours of thick plates begin, by means of anything like the Newtonian doctrines? You well know that whiteness never occurs in periodical colours, except at the beginning of the series, where the thickness is evanescent: and in order to obtain this evanescent measure, we must take the difference between the lengths of the paths of the two portions of light, passing with a given obliquity, and with an obliquity a little different from it; an explanation which is precisely the same with that of the colours which you have lately observed by means of two separate pieces, except that the Newtonian colours of thick plates are formed in light irregularly dissipated, and yours in light regularly reflected only.

You see that I leave polarisation completely out of the question in all my calculations; for, as far as I have been able to discover, the phenomena of polarisation affect light of all colours precisely in the same way, except so far as they happen to separate portions of light from each other, which, in consequence of other circumstances, are capable of exhibiting, when so separated, particular colours; and if the internal reflection which you mention is found to affect some colours exclusively, the phenomenon is altogether singular in this respect. But I look forwards with great interest to the details of your experiments, for at present I can form no very distinct idea of the facts. I must not omit to congratulate you on your happy discovery of the determination of the angle of complete polarisation according to the difference of densities: * it appears to

* In the Philosophical Transactions for 1813, for which the Copley medal was awarded.

be the most important step that has been made in optics since the first discoveries of Mr. Malus, although, like many other great improvements, it seems sufficiently simple and obvious when once known. I shall endeavour to find an opportunity of forwarding to Mr. Knox the copy of your paper which you have sent me for him; it was too late to send it with the separate copies of his paper in the 'Philosophical Transactions.'

Believe me, dear Sir,

Very truly yours,

THOMAS YOUNG.

3.—Dr. YOUNG *to* Sir DAVID BREWSTER *in Reply to Observations on the Priority of some of his Discoveries made likewise by Dr. Seebeck, for which the French Institut had awarded a joint Prize.*

DEAR SIR, London, 24th January, 1816.

I SHOULD have sent an earlier answer to your letter, but I have been waiting to procure the information which you require concerning Seebeck's papers. Unfortunately Dr. Wollaston has mislaid the copy from which my former abstract was made, otherwise I should have begged him to let me send it to you. The coloured plates bear a strong resemblance to your figures, and there is no doubt of the identity of the appearances with those which you have described; there was, however, certainly no mention of heat in that paper. The second paper I have just procured from Dr. Thomson, who is in possession of the whole journal, and in this he has most distinctly proved the dependence of the phenomena on the mode of cooling the glass, so that there cannot be a doubt of his having a parallel claim with you, unless it can be proved that he was previously acquainted with your discoveries, which is indeed barely possible, but not very probable. The number in which his paper is inserted contains an account of the contents of Thomson's Annals for July and August, 1813, only; and it is very improbable that the editor would have inserted this if any English publication of the kind a year later had been current in Germany. Seebeck has also gone a little further in

the investigation than you had done at the time, for he attempts to show that the phenomena do not depend on anything like crystallization, and this opinion is admirably confirmed by the facts which you announce to me at the close of your letter. Your researches are indeed so numerous, and you proceed so rapidly in the career of discovery, that your subsequent labours have often superseded the preceding, and for this reason, as well as for many others, you might perhaps find it more satisfactory in looking back on some of your papers, if they had been a little more compressed in their bulk.* I am perhaps too fond of conciseness myself, but in this instance I am not the only one that regrets the unnecessary detail in some of your papers. It is often true that "half is more than the whole," even of things which are all intrinsically valuable. I must indeed congratulate you on the importance of the new fact respecting the effect of compression and extension in causing a double refraction: perhaps I view it with a partial eye as favourable to my own opinions; but to me it appears to exceed in value the determination of the angle of polarisation, which the Council of the Royal Society fixed on as deserving to be distinguished from the rest of your communications. I have demonstrated in an early number of the 'Quarterly Review,' (though I must beg you not again to quote any anonymous paper as mine,) that every undulation or other impression must be propagated through a minutely stratified substance in the form of a spheroid;† and you have shown by experiment that as soon as the density becomes greater in one direction than in another, which is the condition on which my demonstration proceeds, the propagation actually assumes a spheroidal form: so that this single detached fact is completely explained by my theory, while it has certainly no apparent connexion with any other; at the same time it gives me no assistance with respect to the immediate phenomena of polarisation,

* Sir David Brewster, in his reply, gives very satisfactory reasons for this want of compression in some few of his memoirs. The progress of discovery in this rich field of experimental research, in which many labourers were engaged, was so rapid, that it was necessary, in order to secure priority of claim, to put them upon record without allowing sufficient time for the suppression of much that was superfluous and irrelevant.—*Note by the Editor.*

† See No. XII. p. 230.

strictly so called. I must still wait for a more minute detail of your experiments on internal reflection before I can admit them as properly referable to my law of periodical colours; it is generally necessary, in the application of that law, to suppose the reflection to take place exactly at the surface, in a sense almost strictly mathematical, except that in some cases exactly half an interval appears to be lost, for every kind of rays; and I do not like the idea of supposing even a physical surface to contain an appreciable space. You would, perhaps, be able to throw some new light on this subject by examining more particularly the reflection from the upper part of a soap bubble. You know that a wine-glass dipped into soapy water, and held against a window-shutter, affords the best sort of bubble for the purpose: at the top is a black space almost invisible, but still exhibiting a faint reflection. What has always struck me as the most remarkable about this space is its sharp and very decided termination, whether viewed perpendicularly or obliquely, while according to theory it ought to be gradually shaded off. It might be imagined that the fluid is too thin to allow space for both the reflections which you suppose to take place in some cases; but there are many objections to this explanation: possibly you may find something peculiar in the polarisation of light by this reflection which may illustrate the phenomenon. Biot highly approves the decision of the Royal Society awarding you the Copleian medal, but he thinks the paper particularly pointed out not the most unexceptionable of your communications; he does not, however, bring any material objection against the universality of the law which you have laid down in it, and which is certainly by far the most important part of the paper. I hope you will be able to add from time to time to our knowledge of these subjects, and I could almost wish you could reserve something good for a year or two, that you might have a fair chance for a more valuable medal than the Copleian: unluckily your best discoveries fall within a period when another invention has been made public, which is so striking and so important to society that I suppose it will scarcely fail of overcoming all competition. Have you seen the account of Biot's experiments on

substances causing a polarised ray to revolve round its axis? He must have shown you some of them. I do not fully understand them; but if the fact is such as the simple statement would induce one to suppose, it militates against the doctrine of detached particles; for if the particles had received a rotatory motion round an axis in their passage through the substance they would have retained it, and have turned further round the further they travelled after their emersion. I do not however profess to be sufficiently well acquainted with the phenomena to appreciate the validity of the argument in its whole extent. Pray have the kindness to give the enclosed note to Dr. Duncan and the paper to Playfair, when you have looked at it.

Believe me, dear Sir,
Your faithful, obedient servant,
THOMAS YOUNG.

Thursday.—The Council has adjudged the Rumfordian Medal for the two years ending in November last to Dr. Wells for his Essay on Dew.

4.—*From* Sir D. BREWSTER *to* Dr. YOUNG.

DEAR SIR, Venlaw by Peebles, October 4, 1817.

As you formerly requested me not to mention your name in connexion with any of your anonymous works, I write you at present chiefly to ask if you have any objections to have your theory of the colours produced by the action of crystals upon polarised light mentioned as your own. I have just come to that part of my paper on the laws of polarisation and double refraction, where I mean to introduce the subject.* I have little doubt that it will be found to represent all the phenomena; and my paper will furnish you with the means of putting it to the most decisive test. I have succeeded in reducing the laws of polarisation and double refraction to laws rigorously physical,

* 'On the Laws of Polarisation and Double Refraction in regularly Crystallised Bodies,' read January 13th, 1818; in the Philosophical Transactions for that year, p. 199.

by which the tints and the aberration of the extraordinary ray can, in every case, be computed, whatever be the number, the position, and the character of the axes of extraordinary refraction. These laws are deduced from experiments made upon 60 crystals with two axes, and 20 with one axis. The greater number of the generalisations of Mr. Biot are completely erroneous. His division of crystals into attractive and repulsive is unfounded, and his results respecting the rotation of the luminous particles are mere delusions. . .

. . . . . .

I do not know if you have seen a notice which I sent to Mr. Brande respecting the production of the complementary colours at the separating surfaces of media: I wish very much to have your opinion of this singular result, as it does not appear to me to be explicable upon any theory but the one which I formerly mentioned to you.

I am, dear Sir,

Ever most faithfully yours,

D. BREWSTER.

5.—Dr. YOUNG *to* Sir DAVID BREWSTER.

MY DEAR SIR, Worthing, 10th October, 1817.

I CAN have no objection to your quoting the article in question, as one which *you judge*, from internal evidence, to be mine, if you feel no difficulty in hazarding such an opinion, although, for many reasons, I do not wish to be considered as avowing it *publicly* at present.

I shall be extremely interested in the details of the experiments which you mention, though you do not say by what channel you mean to make them public. I have long been aware that Biot's oscillations were wretchedly visionary, and that some of his experiments were inaccurate; but others I have found to agree extremely well with my calculations, especially those on rock crystal. For the sulphate of lime I have been obliged to suppose, unless his experiments are completely erroneous, that there are in your words "two axes of crystallisation," or some similar irregularity. When I see your

experiments I shall perhaps find in them sufficient data for determining the proportions of the three axes of the distorted spheroid which must, in such cases, be substituted for the surface of revolution; and there will then probably be no great difficulty in calculating the refraction in the Huyghenian manner, if you have not already done it. Do you mean to deny that the spheroid is sometimes oblate and sometimes oblong? which is what Biot's attraction and repulsion must be supposed to mean. Surely this is proved by the interchange of the refractions of two plates of the different substances, placed with their principal sections parallel to each other, and showing the colour dependent on this difference only. I have not seen the note that you sent to Mr. Brande respecting the effects of the common surfaces of different media; and I fear that it will be at least a month before I shall be able to ask him for it. I find that the surfaces of gold and silver exhibit the two species of rings at once; the one by regular reflection, the other by irregular dispersion: and Mr. Arago has shown that the light affording them is differently polarised. This has probably some connexion with the effects that you mention.

I shall be most happy to receive from you at all times any account of your interesting experiments, and of your investigations respecting polarisation; but do not send me any information that you are not prepared to have mentioned again, for I am always scribbling something anonymous; and I am very capable of introducing your experiments where perhaps you would not wish them to appear: but I cannot help it—I can only give you fair warning. I have, indeed, very lately been entering into some optical subjects pretty much at large; but I do not think that I shall resume the consideration of them for a long time.

Believe me, dear Sir,

Yours very sincerely,

THOMAS YOUNG.

6.—Sir D. Brewster *to* Dr. Young.

My dear Sir, Venlaw by Peebles, October 27th, 1817.

I was favoured with your letter of the 10th October, and shall refer to the paper in the 'Quarterly Review' in the way you mention. The manner, however, in which I would have done it would not have been equivalent to your acknowledging it, but would merely have shown my conviction that you were the author. If your theory of the tints gives the same values of them as those deduced from the law, which makes them vary as the square of the sine of the angle which the refracted ray forms with the axis of the crystal, it will suit all crystals, whether they have one or more axes; and if it does not correspond with this law it cannot be correct. Biot's experiments on rock crystal accord with the law, abstracting the effect produced at oblique incidences, which is, however, quite a secondary one, and one which your theory ought not to embrace. In my paper which will be sent in the course of a fortnight to the Royal Society, I have given general methods by which the tint and the velocity of the extraordinary ray can be computed in crystals with any number of axes of extraordinary refraction, and hence not only the three axes, but all the dimensions of what you very properly call the *distorted spheroid*, can be easily ascertained: the ratio of the three axes is easily found. . . . . .

When I mentioned to Mr. Biot, about a year ago, your demonstration, that an undulation propagated through a minutely stratified substance, in which the density is greater in one direction than in another, was spheroidical, he replied that both Laplace and Poisson were of opinion that, in the present state of mathematical analysis, the simplest case of undulation could not be calculated; and therefore that the above theorem was not capable of demonstration. At such a distance from Edinburgh, I cannot command a sight of the volume of the 'Quarterly,' which contains your demonstration, otherwise I should have studied it carefully. I recollect, however, not being able to follow it thoroughly. I am exceedingly interested on this

point, and am, therefore, anxious to know if you are yourself satisfied with the accuracy of the demonstration; and if you can assign any reason why the light which forms the ordinary ray should not also be acted upon by the varying density of the medium?

I am, my dear Sir,
Ever most faithfully yours,
D. BREWSTER.

7.—Dr. YOUNG *to* Sir DAVID BREWSTER.

DEAR SIR, Worthing, 9th November, 1817.

I CANNOT help feeling a little disappointment in not finding in your letter any such confirmation of my theory of periodical colours as I had promised myself, from your having mentioned your experiments as fully agreeing with it, or as superseding those of Biot. You now tell me that my theory ought not to embrace those very particulars to which I have applied it in the paper that you intend to quote with approbation. As to the variation proportional to the square of the sine of the angle made with the axis, in perpendicular incidences, it is so obvious a consequence of the properties of an ellipsis differing little from a circle, upon almost any supposition, that you could scarcely form a theory of double refraction so erroneous as not to give you this law. What I profess to have explained is that very effect of obliquity of incidence which you seem now disposed to refer to another cause. But I hope soon to have the pleasure of seeing the detailed account of your experiments in the paper which you promise the Royal Society.

I conclude that Mr. Biot had the candour to tell you that he had read none of my papers whatever: he promised me that he would attempt it in the course of the summer, but I dare say he has not found leisure. Mr. Laplace has now arrived at so happy a pre-eminence in science, that he thinks it sufficient to assert where others would assign their reasons; and having once asserted, he is not very impatient to retract. He told me in July, as he had often declared before, that the Huyghenian

theory was incapable of determining the relation of the angles of incidence and refraction; and when I could hardly help smiling at the absurdity of the assertion, and endeavoured to prove to him, in three words, how easily and necessarily the law was deduced from the hypothesis, he begged me to send him a short demonstration in writing. I did so, and instead of either admitting it, or endeavouring to point out its deficiency, he now tells me that it is only an "aperçu," a sketch, or a presumption. (Infra, p. 374). This little occurrence is certainly of some value to me, because it spares me a great deal of labour in entering into any further controversy on such a subject with such a person. With respect to Mr. Poisson, when we know how repeatedly and how deeply he has committed himself in praising and in imitating some of Mr. Laplace's least successful speculations, we cannot be surprised at his bearing him out on this point. He praises, for instance, both the theory of capillary attraction and that of oblique refraction as among the highest efforts of human genius, while, to me, they both appear worse than nugatory. Even within the last month I have received a paper from Laplace, which is one of the most amusing instances of waste of labour and calculation that can commonly be met with. It is a determination of the correction required for the length of a pendulum supported by a cylindrical axis, and the ingenious author has exhausted all the powers of his analysis to calculate the motions and rotatory powers of the different parts of the pendulum in their various paths, when he might at once have obtained the same result by simply calculating the curvature of the path of the centre of oscillation from the properties of the epitrochoids.*

Without, however, entering into the discussion of the sufficiency or insufficiency of my demonstration of the *elliptical* form of an undulation propagated through a stratified substance, I can easily explain to you that it must, without all question, assume an *oval* form, which is perhaps enough for your purpose. Mr. Chladni has proved by experiment that the undulations constituting sound are propagated more slowly along a piece of wood

* See Dr. Young's 'Remarks on the Probabilities of Error in Physical Observations,' &c., vol. ii., No. XXVIII.

cut across the grain, than in the direction of the fibre; and you may easily imagine that the velocity in an oblique direction must be intermediate between the greatest and least velocities. You must, therefore, either deny the accuracy of Chladni's experiment, or admit the general fact of an oval undulation; and if you like Mr. Laplace's mode of reasoning, when he says "Nature *takes* the form of the ellipse next to that of the circle," you will have an *aperçu* which he perhaps would like better than my demonstration. I can, however, assign nothing like a reason for the reflection or non-reflection of a ray according to its polarisation.

I agree with you in thinking the experiments, showing the effect of doubly refracting substances on the polarisation of the light by reflection, extremely important; and I shall be glad to know the details of your discoveries, when you have time to prepare them for publication.

Believe me, my dear Sir,
Very truly yours,
THOMAS YOUNG.*

* Sir D. Brewster's paper 'On the Laws of Polarisation and Double Refraction in regularly Crystallised Bodies,' was read to the Royal Society in January, 1818, and was referred by the Council to Dr. Young for examination. Whilst recognising to their fullest extent the great value of the experimental and other results of this celebrated Memoir, he was not satisfied with some of the theoretical views which it contained, and suggested to the author that they should either be withdrawn or modified. A long and animated correspondence ensued between them, which ended with the publication of the entire Memoir, with very few alterations of any moment. The following note was appended to it, at the request of Dr. Young:—

"My dear Sir,

"Your experiments, on the colours afforded by crystals having two optical axes, appear to establish a very important result in the theory of light; for supposing them to be perfectly represented by your general law, it will follow that the tint exhibited depends not on the difference of refractive densities in the direction of the ray transmitted, but on the greatest difference of refractive densities in directions perpendicular to that of the ray. These two conditions lead to the same result, where the effect of one axis only is considered, but they vary materially where two axes are supposed to be combined; and I do not immediately perceive by what modification it will be possible to accommodate the laws of interference to these experiments. There can be little doubt that the direction of the polarisation, in such cases, must be determined by that of the greatest and least of the refractive densities in question; † and it seems to be very possible to apply your mode of calculation to many other phenomena, in which the polarising powers of different crystals are combined.

"Believe me, dear Sir, your very faithful servant,
"THOS. YOUNG."

† *Note added by Sir D. Brewster.*—"This supposition of Dr. Young is perfectly correct. In another paper, which will soon be submitted to the Royal Society, I have given a general method of finding the direction of polarisation for any combination of axes."

8.—*From* M. LAPLACE *to* Dr. YOUNG.

MONSIEUR, Paris, ce 6 Oct. 1817.

J'AI reçu la lettre que vous m'avez fait l'honneur de m'écrire, et dans laquelle vous cherchez à établir que, suivant le système des ondulations de la lumière, les sinus d'incidence et de réfraction sont en rapport constant, lorsqu'elle passe d'un milieu dans un autre. Quelque ingénieux que soit ce raisonnement, je ne puis le regarder que comme un aperçu, et non comme une démonstration géométrique. Je persiste à croire que le problème de la propagation des ondes, lorsqu'elles traversent différens milieux, n'a jamais été résolu, et qu'il surpasse peut-être les forces actuelles de l'analyse. Descartes expliquoit ce rapport constant, au moyen de deux suppositions; l'une, que la vîtesse des rayons lumineux parallèlement à la surface du milieu refringent ne changeoit point par la réfraction; l'autre, que sa vîtesse entière dans ce milieu étoit la même, sous toutes les incidences; mais comme il ne rattachoit aucune de ces suppositions aux lois de la mécanique, son explication a été vivement combattue et rejettée par le plus grand nombre des physiciens jusqu'à ce que Newton ait fait voir que ces suppositions résultoient de l'action du milieu refringent sur la lumière; alors on a eu une explication mathématique du phénomène dans le système de l'émission de la lumière: système qui donne encore l'explication la plus simple du phénomène de l'aberration, que n'explique point le système des ondes lumineuses. Ainsi les suppositions de Descartes, comme plusieurs aperçus de Kepler sur le système du monde, ont été vérifiées par l'analyse: mais le mérite de la découverte d'une vérité appartient tout entier à celui qui la démontre. Je conviens que de nouveaux phénomènes de la lumière sont jusqu'à présent très difficiles à expliquer; mais en les étudiant avec un grand soin, pour découvrir les lois dont ils dépendent, on parviendra peut-être un jour à reconnaître dans les molecules lumineuses des propriétés nouvelles qui donneront une explication mathématique de ces phénomènes. Remonter des phénomènes aux lois et des lois aux forces, est, comme vous le savez, la vraie marche des sciences naturelles.

Monsieur Kater veut bien se charger de vous remettre deux mémoires qui paroîtront dans la prochaine Connaissance des Tems. L'un est relatif à la longueur du pendule, l'autre est une application du calcul des probabilités à la géodésie. Je regarde comme une chose importante, les applications de ce calcul aux sciences, et je désire beaucoup qu'elles se multiplient. Je joins à cet envoi, deux petites additions que j'ai faites à mon 'Essai Philosophique des Probabilités.' Je vous prie de vouloir bien en rendre une à M. Davy, avec mille complimens de ma part, et l'assurance de ma haute estime: ces additions doivent être substituées au lieu des pages 221 et suivantes de l'ouvrage.

M. Kater a bien voulu me communiquer son résultat sur la longueur du pendule à Londres. Ces expériences me paroissent être d'une grande exactitude et faites par un procédé ingénieux. La seule chose qui en puisse laisser quelque incertitude, est le changement de figure du système oscillant dans ses deux états de suspension, en vertu de l'extension de ses parties par les poids qu'elles supportent: il doit en résulter un déplacement dans les centres d'oscillation, qui sans doute est très petit; je suis même porté à croire qu'il est insensible; mais lorsqu'on veut atteindre à la précision d'un cent-millième il est nécessaire d'apprécier toutes les causes d'erreur. J'ai été curieux de comparer le résultat de M. Kater à une formule que M. Mathieu a conclue de l'expérience de Borda, et puis de la comparaison de toutes les expériences du pendule faites jusqu'ici dans les diverses parties du monde, et que j'ai communiquées à M. Kater. Cette formule donna 39^{po}. 13842, pour la longueur du pendule au lieu où M. Kater l'a trouvée de 39^{po}. 1383.

Veuillez, Monsieur, agréer l'assurance de ma considération la plus distinguée.

LAPLACE.

La formule de la longueur du pendule à secondes est 39^{po}. 0819 + 0^{po}. 212923 (sin. lat.) : les pouces sont relatifs à la règle de la Société Royale à la température 62° Fahrenheit.

9.—*From* M. FRESNEL *to* Dr. YOUNG.

MONSIEUR, Paris, le 24 Mai, 1816.

JE vous prie d'agréer l'hommage que je vous fais d'un exemplaire de mon mémoire sur la diffraction. Lorsque je le soumis à l'Institut, je ne connaissais pas vos expériences et la conséquence que vous en aviez tirée, en sorte que je présentai comme neuves des explications que vous aviez déjà données depuis longtemps. Je les ai retranchées dans le mémoire imprimé que j'ai l'honneur de vous envoyer, et je n'y ai laissé que celle des franges colorées des ombres, parce que j'ai ajouté quelque chose à ce que vous aviez déjà dit sur ce phénomène.

Il m'a semblé qu'il fallait supposer un changement d'une demi-ondulation dans les rayons réfléchis par les bords du corps opaque pour que les formules s'accordassent avec les observations. Je n'ai pas pu jusqu'ici me rendre raison de ce retard d'une demi-ondulation, mais la tache centrale des anneaux colorés vus par réflexion présente un fait du même genre qui me paraît tout aussi difficile à expliquer.

La théorie indique que la trajectoire des bandes intérieures sont des hyperboles, et cette conséquence ne vous a point échappé, comme M. Arago me l'a fait voir dans l'explication d'une figure où vous avez représenté leur marche. Les franges extérieures se propagent aussi suivant des hyperboles,* comme je l'ai reconnu, et la courbure de ces trajectoires, qui est nulle pour les bandes intérieures, devient sensible au contraire dans les franges extérieures. C'est une remarque que j'ai eu le bonheur d'ajouter à la vôtre, et que j'ai vérifiée par des observations plus exactes que celles qu'on avait pu faire jusqu'à présent. La démonstration expérimentale de ce fait surprenant, annoncé par la théorie des ondulations, a paru à M. Arago une des preuves les plus frappantes de cette théorie, et une des plus fortes objections contre le système de Newton.

Le moyen d'observation où j'ai été conduit, a de grands avantages sur ceux employés jusqu'à présent, par sa commodité, sa précision et la facilité qu'il donne d'etudier les phénomènes

* See Letter 11, p. 381.

dans des circonstances où ils échappent aux autres procédés. J'espère qu'il engagera les physiciens à s'occuper davantage de la diffraction, dont vous avez tiré le premier des preuves si évidentes de la théorie des ondulations.

En interceptant la lumière d'un côté du corps opaque, vous avez fait voir que les bandes intérieures provenaient de la rencontre des rayons infléchis par ses deux bords. Vous avez encore démontré l'influence des rayons lumineux les uns sur les autres, en faisant passer la lumière à travers deux petits trous très voisins, et en formant de cette manière des bandes semblables à celles qu'on observe dans l'intérieur des ombres. Il me semble qu'on ne peut faire aucune objection raisonnable aux conséquences que vous avez tirées de cette belle expérience.

Néanmoins, pour éloigner toute idée de l'action des bords du corps, de l'écran, ou des petits trous, dans la formation et la disparition des franges intérieures, j'ai cherché à en produire de semblables au moyen du croisement des rayons réfléchis par deux miroirs, et j'y suis parvenu apres quelques tâtonnements. J'ai remarqué que ces franges étaient toujours perpendiculaires à la ligne qui joignait les deux images du point lumineux, et que leur direction était indépendante de celle des bords des miroirs. D'ailleurs les rayons qui arrivaient à mon œil après avoir traversé la loupe, étaient partis de points très éloignés du bord commun des deux miroirs, et avaient été réfléchis régulièrement. En mesurant la largeur de ces franges nous avons trouvé, M. Arago et moi, qu'elle s'accordait parfaitement avec celle déduite par la théorie de l'angle que faisaient entr'eux les deux rayons visuels dirigés sur les deux images du point lumineux.

M. Arago a donné les détails de cette expérience dans le tome 1er des Annales de Physique et de Chimie, mois de Mars, 1816.

J'ai fait voir dans mon mémoire que, sur un même point d'une surface très étroite ou d'une grande convexité, les mêmes rayons incidens peuvent être réfléchis dans des directions différentes. Mais cela ne suffit pas pour expliquer les images colorées réfléchies par des cilindres métalliques d'un petit diamètre, parce qu'on peut en dire autant de tous les points de

leur surface, en sorte que les diverses couleurs résultant du croisement des ondulations se superposent et se confondent, à moins que des aspérités ou des raies n'interrompent la continuité de la surface. En répétant dernièrement l'expérience de Dutour, je me suis assuré que les images colorées provenaient de quelques raies longitudinales, comme le pensait M. Arago, car en faisant tourner le fil métallique sur son axe, j'ai vu ces images changer de place. Je l'ai fait polir ensuite au tour avec soin, de manière à bien effacer les raies longitudinales, et il n'a plus réfléchi qu'une lumière continue légèrement irisée dans le sens perpendiculaire à l'axe.* La grande convexité de ces cilindres en isolant les raies favorise le développement des couleurs, et c'est là probablement la principale cause du phénomène. Quand on croit avoir fait une découverte, on n'apprend pas sans regret qu'on a été prévenu, et je vous avouerai franchement, Monsieur, que c'est aussi le sentiment que j'ai éprouvé, lorsque M. Arago m'a fait voir qu'il n'y avait qu'un petit nombre d'observations véritablement neuves dans le mémoire que j'avais présenté à l'Institut. Mais si quelque chose pouvait me consoler de n'avoir pas l'avantage de la priorité, c'était de m'être rencontré avec un savant qui a enrichi la physique d'un si grand nombre de découvertes importantes, et cela n'a pas peu contribué en même temps à augmenter ma confiance dans la théorie que j'avais adoptée.

Je suis, avec la plus haute considération, Monsieur,

Votre très-humble et très-obéissant serviteur,

FRESNEL.

10.—*From* M. ARAGO *to* Dr. YOUNG.

MONSIEUR, Paris, le 13 Juillet, 1816.

J'AI l'honneur de vous adresser quelques exemplaires d'un mémoire sur la diffraction de la lumière, que j'ai fait insérer dernièrement dans le nouveau journal que nous rédigeons, M. Gay-Lussac et moi, sous le titre *d'Annales de Chimie*

* See Letter 11, p. 382.

et de Physique.' L'auteur, M. Fresnel, ne connoissait pas, quand il l'a composé, les excellens écrits que vous avez publiés, sur cette matière, dans les 'Transactions Philosophiques.' Vous verrez que depuis que je lui en ai fait part il s'est empressé de vous rendre justice et de reconnaître l'antériorité de vos titres.

Le mémoire de M. Fresnel me paraît devoir être considéré comme la démonstration de votre doctrine *des interférences.* Je ne vois pas trop, en effet, comment les partisans du système de l'émission pourront expliquer les trajectoires courbes des bandes diffractées; ou plutôt, je devine déjà, que pour ne pas abandonner la route qu'ils ont suivie jusqu'à présent, ils révoqueront ce fait en doute, ou s'abstiendront d'en parler. Si le volumineux ouvrage que M. Biot vient de publier sous le titre de 'Traité de Physique expérimentale et mathématique' est déjà parvenu jusqu'en Angleterre, vous aurez eu l'occasion de remarquer, par quels argumens pitoyables il prétend prouver, contre votre opinion, que deux faisceaux lumineux, qui se croisent, n'exercent jamais l'un sur l'autre aucune influence sensible. J'aurai, sous peu, l'occasion de m'occuper de cet objet; en attendant j'ai inséré dans nos 'Annales' deux notes qui mettront le public au courant de la question, et qui renferment un aperçu de vos ingénieux travaux. L'une d'elles est relative à l'expérience de la *disparition* des bandes intérieures que vous avez publiée dans les Transactions Philosophiques pour 1803, et à laquelle j'ai fait une modification que me paraît importante par les conséquences qui s'en déduisent. Cette modification consiste en ceci: que la disparition de la totalité des bandes diffractées, qui se forment dans l'intérieur de l'ombre d'un corps opaque, a également lieu lorsqu'on substitue un verre *diaphane* d'une certaine épaisseur à l'écran *opaque* dont vous vous serviez. Ceci conduit à un moyen extrêmement précis, pour mesurer les plus petites différences de réfraction; je le mettrai bientôt en pratique, et j'ai tout lieu d'espérer qu'il réussira même pour les substances gazeuses. Dans tous les cas, ces considérations auront toujours à mes yeux un grand prix, puisqu'elles ont été le prétexte de cette lettre et qu'elles m'auront fourné l'occasion de vous

présenter les assurances de la profonde estime que vos travaux m'ont inspiré depuis long temps.

Votre très-humble et très-obéissant serviteur,

F. ARAGO.

Cette lettre vous sera remise par M. Dupin, l'un de nos ingénieurs les plus distingués. Mon excellent ami, M. de Humboldt, qui a eu l'an dernier l'honneur de faire votre connaissance, s'est chargé de vous le recommander.

11.—*From* Dr. YOUNG *to* M. ARAGO.

MY DEAR SIR, London, 48, Welbeck-street, 12th January, 1817.

I HAVE long been intending to scold you for leaving England without performing your promise of paying me another visit with your friend Gay-Lussac at Worthing. I was the more mortified at the circumstance, because I fear that you left me under a mistaken apprehension that I had some engagement for the day which made your company inconvenient to me; —this was very far from the truth;—and when I expressed some regret that you had not written to give me notice of your coming, it was more from feeling how easily it might have happened that I might have been absent the whole of the afternoon without seeing you, than from any partial engagement which I had actually made. For the present, the obligation is all on my side. I am sensible how great a compliment you paid me in undertaking such a journey for such an object; and I am conscious that I was unable to repay you either by information or civilities of any kind. You were already acquainted with everything that I meant to have told you respecting my optical speculations; and you did not give me time to do the honours of the country by common hospitality. I am, however, most happy to find that you are to return in the spring, and then, I trust, that you will allow me to make up for the deficiency.

I was reflecting, after you left me, on the very important experiment which you made on the equality of the intensity of colours formed in reflected and in transmitted light: you seemed to regard it as forming a difficulty in my hypothesis; but in reality there is nothing in this fact at all unfavourable to that theory, although it requires some modification of the general law of interference, if we set out with considering the light as arriving at any given point independently of the action of this law; for instance, in the present case of transmitted light, *after two* internal reflections, which would leave it less intense than you actually found it. But it is equally consistent with the theory to consider the colour in question as being formed at the instant of the second reflection; and the analogy with elastic bodies fully justifies this mode of applying the law, so as to consider the whole light once reflected, as interfering with an equal portion of the transmitted light. (Supra, p. 160.)

The same analogy is fully sufficient to explain the inversion of the undulation, or the loss of half an interval, when a direct partial reflection takes place from the surface of a rarer medium, as, I believe, you are yourself aware. But Mr. Fresnel, in his letter to me, mentions this fact as equally inexplicable with the inversion by extremely oblique reflection. I am sincerely delighted with the success which has attended Mr. Fresnel's labours, as I beg you will tell him; and I think some of his proofs and illustrations very distinctly stated; but I cannot fully adopt your expression in the letter you wrote by Mr. Dupin, that his memoir may be "considéré comme *la* démonstration de la doctrine des interférences;" for neither I nor any of those few who were acquainted with what I had written can find a single *new* fact in it of the least importance: nothing certainly half so important as your experiments on the colours seen in transmitted light, or on the non-interference of light polarised in opposite directions. Mr. Fresnel's words, in his letter, are "les franges extérieures se propagent aussi suivant des hyperboles comme je l'ai reconnu, et la courbure de ces trajectoires, qui est nulle pour les bandes intérieures, devient sensible au contraire dans les franges extérieures." Now you are all well aware that this was known to Newton himself, and

that he attempted to elude the difficulty by saying that the light was not the same; and it was, therefore, unnecessary for me to repeat it in the same form. And the precise hyperbolical nature of the curves concerned is by no means a very strong point in the chain of evidences, partly on account of the difficulty of measuring the exact breadth of the fringes, and partly on account of the loss of the half interval, not hitherto explained. Mr. Fresnel has repeated some of Mr. Dutour's experiments on small cylinders, and has very truly observed that the spectra move with the cylinders. This was the reason that I never considered these experiments as of any value, the circumstance having been noticed by several authors, and, among the rest, by Mr. Brougham in 1796.

We have made but little progress in the measurement of the pendulum, except that Major Kater's experiments are nearly completed. Troughton is going on with his, but I am persuaded they can be of no use, from the nature of his suspension. I have been calculating the effect of the flexure of a spring in shortening the pendulum, and I find that it must be very sensible in all imaginable cases, even when the elastic force of the spring as an impelling power is wholly inconsiderable. I hope in a few weeks to get a clockmaker to make a scapement for my pendulum, which shall not have any influence on its rate; or if otherwise, to make the experiments without a scapement, as has been done in other instances; but in this case it would be necessary to fix the moveable weight at such points as would afford coincidences at convenient intervals, and the whole determination would be more laborious.

I have been reconsidering the theory of capillary attraction, and have at last fully satisfied myself with respect to the fundamental demonstration of the general law of superficial contraction, which I have deduced in a manner at once simple and conclusive from the action of a cohesive force extending to a considerable number of particles within a given insensible distance. This solution has very unexpectedly led me to form an estimate, something more than merely conjectural, though not fully demonstrative, of the magnitude of the ultimate atoms of bodies; of water, for instance, about a million of which

would occupy a length equal to the diameter of one of the red particles of blood. This, however, you may possibly regard as a mere dream, and you are fully at liberty to do so.

I have also been reflecting on the possibility of giving an imperfect explanation of the affection of light which constitutes polarisation, without departing from the genuine doctrine of undulations. It is a principle in this theory, that all undulations are simply propagated through homogeneous mediums in concentric spherical surfaces like the undulations of sound, consisting simply in the direct and retrograde motions of the particles in the direction of the radius, with their concomitant condensation and rarefactions. And yet it is possible to explain in this theory a transverse vibration, propagated also in the direction of the radius, and with equal velocity, the motions of the particles being in a certain constant direction with respect to that radius; and this is a *polarisation.** But its inconceivable minuteness suggests a doubt as to the possibility of its producing any sensible effects: in a physical sense, it is almost an evanescent quantity, although not in a mathematical one. Its foundation is this: suppose two particles to reflect two portions of light, which interfere with each other, and form a dark fringe, the one being situated at the distance of several intervals from the other, in a direction transverse to that of the fringe: it is obvious that their interference can never be so completely effectual as not to leave some remains of the motions combined with each other; the direct motion of the one will destroy the retrograde motion of the other: but the transverse motions of each, with respect to the line bisecting their directions, will conspire with each other and will produce a single transverse vibratory motion. And who shall say that this motion will be too minute to produce any effect in any circumstances?

Pray give my compliments to Mr. Gay-Lussac, and tell him that I was much disappointed in not having some further conversation with him on elective attractions. Mrs. Y. begs to unite with me in kind remembrances both to him and to your-

* This suggestion was a capital step in the undulatory theory of light. See Dr. Whewell's 'History of the Inductive Sciences,' vol. ii. p. 417.

self. I am happy to hear that the work on Egypt is going on, and that Mr. Jomard has married a pretty widow.

Believe me, dear Sir,
Very sincerely yours,
THOMAS YOUNG.

I know that I need not apologize to you for writing in English, as you read it with so much ease. I write it so much faster than French, and of course better in a *duplicate ratio*, although this is of less consequence.

12.—Dr. YOUNG *to* M. ARAGO.

MY DEAR SIR, London, Welbeck-street, 22nd April, 1817.

I HAVE been preparing for you a few memorandums on the subject of the double internal reflection of light, which I promised you at Paris, but I have not had time to put them together in a very satisfactory form: having, however, an opportunity of writing, I do not like to let it pass without assuring you that I have not forgotten the many pleasant hours that I spent at Paris, and the many kindnesses that I received there, for which I am indebted to no one so much as to yourself. Pray tell me if I am not to expect the pleasure of seeing you here shortly; I understand that Mudge is ready to co-operate in everything of every kind that you can desire of him.

With respect to the reflections in question, you were very right in hesitating to admit the matter as self-evident, and I was a little precipitate in my way of stating it: for I find upon calculation, that if we considered the simple velocities as the measure of the intensity of light, the second reflection would be no stronger in the case in question than in any other case, and the transmitted light ought to be much less strongly coloured than the reflected, since the arrival of a new vibration at the first surface would not affect the *velocity* of the second internal reflection, as added to or subtracted from the velocity produced by its simple transmission. But the velocity alone is not the measure of the intensity of light; and it must in

general be considered as proportional to the square of the velocity; consequently the degree in which the transmitted light is strengthened or weakened by the interference with the second reflection, must depend in some measure upon its own intensity, and upon the density of the mediums concerned, and for a substance nearly of the refractive density of glass. The intensity of the colours seen in the transmitted light will become nearly or quite equal to that of the colours seen in the reflected light, although the proportion will vary a little in different cases. I do not know whether you will be quite satisfied with this result, but it seems to me at least to assist us a little; as for the inversion, or the loss of half an interval between the two reflections, there is no difficulty whatever, as you will see when I send you the calculation which I have not had time to copy out. Pray present my kind compliments to Madame Arago, and believe me ever yours very sincerely,

THOMAS YOUNG.

13.—*From* Dr. YOUNG *to* M. ARAGO.

MY DEAR SIR, Worthing, 15th September, 1817.

I HAVE to thank you for the pleasure I have received in looking over the numbers of your Annals, a work which appears to me to be admirably conducted. I am in daily expectation of receiving the last cahier, which I believe is at my house in London. I was rather surprised that you inserted Mr. de Prony's paper on the mètre, without adding the remark "that the English standards being always employed at 62°, the true length of the mètre in English feet is not the number which he has set down, but 39.3710 as I had made it," or perhaps .0008 more, according to some late experiments of which Major Kater will tell you more. He will also show you my letter to Mr. Laplace, on the subject which I mentioned to you, if you think it worth reading, before he delivers it.

I have been amusing myself lately with revising some of my investigations respecting light: but I do not know that I have made out anything new that is very important: you will, how-

ever, be interested in the result of a calculation which completely solves your difficulty respecting the transmitted and reflected rings. In the first place there is no doubt that the intensity of light must be measured by the squares of the velocities of the particles, and not by the simple momenta, otherwise there would be an increase of the whole existing quantity of light after every partial reflection: and in the second place you will find that the difference in the squares of the velocities of the compound transmitted undulations, at the distance of half an interval, and a whole interval, is equal to the difference of the squares in the case of reflection, except a slight diminution exactly equal to that which would be produced by viewing these last through the plate in question: and possibly in the case of oblique incidences, even this difference would be found to vanish.

I do not know whether it has occurred to you that the difference between the dimensions of the rings discoverable upon silver as you first observed, from the light irregularly reflected, and the ordinary rings, is perfectly intelligible from the circumstance of the difference of the interval of retardation in cases of oblique incidence, the light not passing necessarily through the plate in the same angle before and after its reflection. Have you observed that steel reflects regularly a series of rings with a black central spot, and gold ditto with a white one?

I cannot yet satisfy myself respecting the true explanation of Biot's experiments on oil of turpentine, and I shall be glad to receive Mr. Fresnel's which you mentioned to me, as soon as he is ready to make it public. In short, the relation of Biot's experiments is so mixed with his theory, that I am very much at a loss to separate them.

Are we to entertain any hopes of seeing you this autumn? if not, you must come in the spring, and bring Madame Arago: we will take care of her while you are engaged in Norfolk. Pray give my compliments to Madame Biot when you see her. I do not know what Sir C. Blagden has done respecting the compass.

Ever yours,

THOMAS YOUNG.

14.—*From* M. ARAGO *to* Dr. YOUNG. *No date or place.*

MON CHER ET ILLUSTRE CONFRÈRE,

JE vous adresse, ci-joints, les remercîmens de MM. Haüy, Prony et Poisson, au sujet de leur nomination comme membres étrangers de la Société Royale de Londres.* Permettez-moi, en mon particulier, de vous prier de joindre aux bontés sans nombre, que vous avez déjà eues pour moi, celle d'être auprès de votre respectable Président et de vos confrères, l'interprète de ma profonde gratitude.

* * * * *

J'ai un calque, de grandeur naturelle, que M. Jomard a fait faire, à ma prière, de l'inscription hiéroglyphique que vous désirez connaître. J'attends une occasion favorable pour vous le faire passer. Le rouleau est trop volumineux pour que je puisse le confier à la poste.

Je vous remercie des détails que vous avez bien voulu me donner au sujet de la théorie des ondes ; vous savez que je suis un de ses plus zélés prosélytes : à ce titre je vois avec peine qu'elle se complique un peu. Il y a toujours la dispersion de la lumière et la polarisation qui attendent une explication plausible. C'est à vous, dont la théorie des interférences portera le nom dans la postérité, qu'il appartient de lever ces deux grandes difficultés.

Permettez moi, avant de terminer cette lettre, de vous demander des nouvelles de vos observations du pendule. M. de Humboldt entre dans ce moment chez moi, et me charge de vous faire ses complimens.

Adieu, Monsieur ; présentez, je vous prie, mes respects à Madame Young, et recevez les nouvelles assurances du bien sincère attachement de votre très humble serviteur,

P. ARAGO.

* Messieurs Arago, Haüy, Prony and Poisson were elected Foreign Members of the Royal Society in 1818. This letter of acknowledgment was written therefore in *that* year.—*Note by the Editor.*

15.—*From* Dr. YOUNG *to* M. ARAGO. *For the 'Annales' if you think proper.*

MY DEAR SIR, Worthing, 4th August, 1819.

YOU will imagine how greatly I have been interested with the two principal papers in the 'Annales de Chimie' for May. Perhaps, indeed, you will suspect that I am not a little provoked to think that so immediate a consequence of the Huyghenian system, as that which Mr. Fresnel has very ingeniously deduced, should have escaped myself, when I was endeavouring to apply it to the phenomena in question: but in fact, I am still at a loss to understand the possibility of the thing; for if light has at all times so great a tendency to diverge into the path of the neighbouring rays, and to interfere with them as Huyghens supposes, I do not see how it escapes being totally extinguished in a very short space, even in the most transparent medium, as I have observed in my first paper on the subject (*supra*, p. 149): I cannot, however, deny the utility of Mr. Fresnel's calculations. I have not yet seen his analysis; but the result may easily be exhibited in a very simple form, by merely considering the effect of a pencil passing through a small circular orifice, each point of which contributes equally to furnish light to an object situated in the axis of the pencil: for, supposing the area of the orifice to be x, the difference of the paths of the rays passing through its centre and its circumference will obviously vary as x, both these quantities being as the square of the diameter; we have also dx for the fluxion of the area depending on its annular increment, and belonging to the difference in the paths expressed by $\frac{a}{d}\,x$, d being the distance of the object and a constant quantity; so that the fluxion of the intensity of the light will be $\cos\left(\frac{a}{bd}\,x\right)dx$,* supposing the law of the undulations to be that of the cycloidal pendulum, which is the simplest possible; consequently the intensity for an orifice, of which the area is any finite quan-

* Note the correction of this in the next letter (16), p. 391, as well as Dr. Young's reply, No. 17.—*Note by the Editor.*

tity x will be c sin. $\frac{a}{bd}\ x$ which will vanish when $\frac{a}{d}\ x$ becomes equal to the breadth of a complete undulation; a result equivalent to the apparent inversion of the undulation by oblique reflection, which I observed, but confessed myself "unable to explain."

Believe me, my dear Sir,
Ever most truly yours,
THOMAS YOUNG.

Pray have the goodness to take charge of the enclosed for Mrs. Kater, whose direction I do not know. I shall send Mr. Jomard some hieroglyphics by Mr. Dupin: I have had some late accounts from Egypt, confirming my interpretations.

16.—*From* M. FRESNEL *to* Dr. YOUNG.

MONSIEUR, Paris, le 19 Septembre, 1819.

J'AI l'honneur de vous adresser deux exemplaires de mon mémoire sur la diffraction, tel qu'il vient d'être imprimé dans les 'Annales de Physique et de Chimie.' Il ne pouvait pas y être inséré en totalité à cause de son étendue; mais la partie supprimée ne contenant guère que des objections contre le système Newtonien, vous aurait présenté peu d'intérêt. L'extrait publié contient la partie essentielle de mon mémoire: la théorie de la diffraction et sa vérification expérimentale. Cette théorie, comme vous l'avez très bien dit, n'est autre chose que le principe de Huyghens appliqué aux phénomènes en question. Sans doute ce grand géomètre en aurait aisément déduit les loix de la diffraction, s'il avait songé à l'influence mutuelle que des ondes produites par un mouvement oscillatoire doivent exercer les unes sur les autres. Mais il vous était réservé d'enrichir la science du principe fécond des interférences, et de montrer par une foule d'applications ingénieuses de quelle utilité il pouvait être en optique.

Le principe d'Huyghens me paraît, aussi bien que celui des interférences, une conséquence rigoureuse de la coëxistence des petits mouvemens dans les vibrations des fluides. Une onde

dérivée peut être considérée comme l'assemblage d'une infinité d'ébranlements simultanés; on peut donc dire, d'après le principe de la cöexistence des petits mouvemens, que les vibrations excitées par cette onde dans un point quelconque du fluide situé au-delà, sont la somme de toutes les agitations qu'y aurait fait naître chacun de ces centres d'ébranlement en agissant isolément. A la vérité, d'après la nature des ondes dérivées, ces centres d'ébranlement ne peuvent pas produire de mouvement rétrograde, et les ondulations élémentaires qui en émanent ne sauraient avoir, dans des directions obliques à l'impulsion primitive, la même intensité que suivant la normale à l'onde génératrice. Mais il est évident que le décroissement d'intensité doit suivre une loi de continuité, et peut être considéré comme insensible dans un intervalle angulaire très petit: or, cette considération suffit pour la solution du problème; car dès que l'inclinaison des rayons est un peu prononcée, il est aisé de voir qu'ils se détruisent mutuellement.

Mais comment ces destructions mutuelles n'affaiblissent-elles pas considérablement la lumière totale? C'est une conséquence générale des vibrations des fluides élastiques que la somme des forces vives reste toujours constante, de quelque manière que l'on subdivise et recompose le mouvement. On peut aisément vérifier ce principe dans le cas très simple des bandes obscures et brillantes produites par l'interférence de deux systèmes d'ondes lumineuses réfléchies sur deux miroirs légèrement inclinés entr'eux; qui sont d'une intensité sensiblement uniforme dans le petit espace angulaire où se forment les franges. On trouve en intégrant, que la somme des forces-vives d'une demi-frange, depuis le point le plus sombre de la bande obscure, jusqu'au point le plus éclatant de la bande brillante, est précisément la même que dans les deux systèmes d'ondes supposés indépendans l'un de l'autre, malgré la destruction de mouvement qui résulte de leur influence mutuelle dans les points de discordance; parce-qu'elle est exactement compensée par l'augmentation de mouvement dans les points où leurs vibrations s'accordent. En effet si l'on représente par a et a' les intensités des vîtesses d'oscillation que les deux séries d'ondes imprimeraient aux molécules éthérées, en agissant isolément, on a pour l'expression de l'in-

tensité d'oscillation du système d'ondes résultant du concours des deux autres $\sqrt{\left\{a^2 + a'^2 + 2aa' \cos\left(2\pi . \frac{x}{c\lambda}\right)\right\}}$, 2π représentant la circonférence, dont le rayon est 1, λ la longueur d'ondulation, et $\frac{x}{c}$ la différence des chemins parcourus dans le point de la frange que l'on considère ; j'indique ici par x la distance de ce point à celui d'accord parfait, c'est-à-dire au point le plus éclairé de la bande brillante : $\frac{1}{c}$ est le rapport constant de cette distance à l'intervalle correspondant entre les deux systèmes d'ondes. La force vive étant la masse multipliée par le carré de la vîtesse sera proportionnelle à $a^2 + a'^2 + 2aa' \cos.\left(2\pi . \frac{x}{c\lambda}\right)$, et sa différentielle à $\left\{a^2 + a'^2 + 2aa' \cos.\left(2\pi . \frac{x}{c\lambda}\right)\right\} dx$, dont l'intégrale est $(a^2 + a'^2)\, x - \frac{c\lambda}{2\pi} . 2aa \sin.\left(2\pi . \frac{x}{c\lambda}\right)$, qui devient $(a^2 + a'^2)\, x$, lorsque $\frac{x}{c\lambda}$ égal à $\frac{1}{2}$, c'est-à-dire, lorsqu'on intègre depuis le point d'accord parfait jusqu'à celui où les deux systèmes d'ondes diffèrent d'une demi-ondulation : or, d'après la même notation $(a^2 + a'^2)$, x est précisément la somme des forces vives que les deux systèmes d'ondes apportent dans cet intervalle d'une demi-frange, abstraction faite de leur interférence ; l'influence mutuelle qu'ils exercent l'un sur l'autre ne diminue donc pas la somme des forces vives.

Dans le calcul qui termine votre lettre à M. Arago, où vous appliquez le principe d'Huyghens au cas d'une ouverture circulaire, il me semble, si je comprends bien votre notation, que vous vous êtes mépris sur la formule d'interférence ; le fluxion de l'intensité de la lumière dans le point qui répond au centre de l'ouverture n'est pas $\cos.\left(\frac{a}{ld}\, x\right) dx$, mais $\cos.\left(\frac{1}{2}\, \frac{a}{ld}\, x\right) dx$, dont l'intégrale est $2c \sin.\left(\frac{1}{2}\, \frac{a}{ld}\, x\right)$. Et en effet, cette expression, qui devient nulle quand $\frac{a}{d}\, x$ est égal à l, comme celle que vous obtenez $c \sin.\left(\frac{a}{ld}\, x\right)$, s'accorde encore avec l'expé-

rience en ce qu'elle atteint son maximum lorsque $\frac{a}{d}x$ est la moitié de l; tandis que c sin. $\left(\frac{a}{ld}\ x\right)$, devient alors une seconde fois égal à zéro, et ne peut pas en conséquence représenter l'intensité de la lumière dans la projection du centre d'une ouverture circulaire.

Il est aisé, sans le secours de l'analyse et par une considération géométrique bien simple, de déterminer les circonstances de maximum ou de minimum de lumière pour le point dont il s'agit. Il suffit de diviser par la pensée la surface de l'ouverture circulaire en anneaux concentriques dont les circonférences répondent à des différences d'une demi-ondulation dans les chemins parcourus ; ces anneaux étant égaux au surface envoient chacun le même nombre de rayons, et comme ces rayons sont sensiblement égaux en intensité, d'après mon hypothèse, il est clair qu'ils se détruisent tous mutuellement quand les anneaux sont en nombre pair, et qu'ils doivent produire au contraire par leur réunion la lumière la plus vive possible, lorsque les anneaux sont en nombre impair.

D'après cette manière d'envisager les phénomènes de la diffraction, il n'est plus nécessaire de supposer une inversion de l'ondulation dans les rayons réfléchis sur le bord de l'écran, qui ne sont qu'une très petite partie de ceux qui concourent à la production des franges.* Mais je n'en crois pas moins à cette inversion, du moins dans la réflexion produite par les corps parfaitement transparens, tels que l'eau, le verre, &c. Cette opinion est fondée sur une hypothèse à laquelle j'accordais la préférence depuis longtemps, et que je viens de vérifier par des expériences qui me paraissent décisives : je ne crois pas que la réflexion soit occasionnée par une plus grande densité de l'éther dans le milieu réfringent, mais par des réflexions partielles sur les particules propres du milieu, que je suppose dans une petite épaisseur de la surface participer à la fois aux vibrations des rayons transmis et des rayons réfléchis. Il est aisé de concevoir

* This was the correction of an important inaccuracy in Dr. Young's explanation of the external fringe of shadows in diffraction, to which M. Fresnel's first explanation (letter 9, p. 376) was equally liable. See Dr. Whewell's 'History of the Inductive Sciences,' vol. ii. p. 406. See also the next letter, No. 17.—*Note by the Editor.*

comment la réflexion devient insensible à une certaine distance de la surface, lorsque les intervalles qui séparent les particules du milieu sont très petits par rapport à la longueur d'une ondulation, puisqu'alors toutes les réflexions élémentaires se détruisent mutuellement dans l'intérieur du corps.

Je vous prie d'avoir la bonté d'offrir de ma part à la Société Royale de Londres un des exemplaires ci-joints de mon mémoire sur la diffraction.

J'ai l'honneur d'être avec la plus haute considération,

Monsieur,

Votre très-humble et très-obéissant serviteur,

A. FRESNEL.

17.—*From* Dr. YOUNG *to* M. FRESNEL.

Worthing, 16th October, 1819.

JE vous remercie infiniment, Monsieur, pour le présent que vous m'avez fait de votre beau mémoire, qui mérite assurément un rang distingué parmi les écrits qui ont le plus contribué aux progrès de l'optique. Je n'ai pas la moindre idée d'insister sur l'opération des rayons réfléchis des bords d'un corps opaque ; je savais même très bien, que quand on se sert de deux fentes parallèles, il faut se rapporter au milieu de chacune pour l'interférence, comme vous pouvez voir dans la figure 442 de mes Lectures ; mais je n'avais jamais eu l'heureuse idée d'analyser les résultats de la combinaison des ondulations particulières, qui vous a si bien réussi, et ce qui m'en a empêché, c'est la difficulté que je sentais d'apprécier assez justement l'effet de l'obliquité, que vous n'avez pas trouvé nécessaire de comprendre dans votre calcul. J'avoue que ma petite lettre à M. Arago manque d'exactitude, et j'espère qu'il ne l'aura pas publiée ; j'ai regardé la chose trop à la hâte ; et comme le seul résultat que je me sois donné la peine d'examiner était d'accord avec les vôtres, et avec l'expérience, je m'en suis trop aisément satisfait. Mais vous verrez par la petite table des marées que je vous adresse avec cette lettre, que la vraie manière d'envisager la combinaison des ondulations m'était assez familière ; en effet, nous n'avons qu'à diviser le trou circu-

laire en petits anneaux concentriques d'une égale aire, qui répondront à des différences égales dans les routes, et il s'ensuit du principe connu de la combinaison des ondulations, dont je me suis servi dans cette construction, pour les marées, que si l'on représente les petites ondulations égales par les côtés d'un polygone, inscrit dans un cercle, et formant les angles extérieurs égaux aux distances des ondulations sur le cercle qui les mesure, les cordes de ces polygones ou des arcs qui les représentent dans leur dernier état, seront proportionnelles aux grandeurs des ondulations composées, et voilà les doubles sinus des moitiés des angles, auxquels vous êtes parvenu.

Huyghens aurait pu sans doute, comme vous remarquez, soupçonner ce qui serait l'effet de l'interférence des ondulations, mais il ne paraît pas qu'il ait eu aucune idée de ce qui pouvait constituer la différence des couleurs, quoiqu'il aurait pu adopter la suggestion de Newton ou de Malebranche que j'ai citée, s'il avait poursuivi plus loin ses recherches. J'avais remarqué que l'interférence de deux systèmes quelconques d'ondulations n'altérait pas la somme des forces vives, et je vois que M. Poisson a démontré quelques uns de mes résultats appartenant à l'intensité de la lumière d'une manière plus directe, dans un mémoire qu'il a eu la bonté de m'adresser. Si vous le voyez je vous prie de l'en remercier de ma part, et de lui dire, qu' il trouvera dans les Mémoires de l'Académie de Berlin, pour 1775, des expériences de Lambert sur les flûtes, comparées avec la théorie de Bernouilli, qu'il ne paraît pas connaître.

La polarisation nous présente encore beaucoup de difficultés. Je m'étais flatté, d'après ce que M. Biot venait d'annoncer dans son mémoire, que vous en aviez levé la plupart, et que vous aviez expliqué la rotation apparente des rayons dans quelques fluides que M. Biot a découverte : si cela est vrai, je vous serais extrêmement obligé si vous pouviez me donner quelque idée de votre théorie. Vous trouverez quelques mots sur l'optique dans l'extrait astronomique que j'ai l'honneur de vous adresser.

Je suis, Monsieur, avec les sentimens les plus distingués,

Votre très-humble et très-obéissant serviteur,

THOMAS YOUNG.

18.—*From* M. Fresnel *to* Dr. Young.

Paris, 18 Février, 1823.

. . . . . .

J'avais remarqué depuis six ans que deux réflexions totales, dans l'intérieur du verre, ou d'un autre corps transparent, peuvent, sous une incidence convenable, imprimer à la lumière polarisée dans l'azimut de 45°, la modification que j'appelle maintenant *polarisation circulaire*, et que je définissais par la réunion de deux systèmes d'ondes égaux en intensité, polarisés à angle droit, et distans l'un de l'autre d'un quart d'ondulation. J'avais observé aussi que cette différence de marche entre les deux systèmes d'ondes, dont l'un est polarisé suivant le plan de réflexion et l'autre suivant un plan perpendiculaire, variait avec l'inclination des rayons et devenait nulle aux deux limites de la réflexion totale ; mais il me semblait bien difficile de découvrir suivant quelle loi, et je ne l'avais pas même essayé ; ce n'est que depuis très peu de tems que je me suis occupé de ce problème ; j'en ai trouvé la solution beaucoup plus vîte que je ne m'y attendais. J'ai lu à ce sujet un nouveau mémoire à l'Institut, dans lequel je fais voir d'abord comment on peut arriver aux formules d'intensité des rayons réfléchis sous des incidences obliques, en partant de la loi de Descartes, et s'appuyant seulement sur le principe de la conservation des forces vives et sur une hypothèse mécanique très simple et très admissible, dont je n'ai pas donné la démonstration, à la vérité, mais qui me paraît facile à établir. C'est à l'aide de ces formules que je suis parvenu à découvrir la loi dont je viens de parler : elles avaient été publiées en 1821, dans les 'Annales de Chimie et de Physique,' à la fin de la dernière note ajoutée au rapport de M. Arago sur la coloration des lames crystallisées : j'avais fait voir comment elles servent, non seulement à calculer les proportions de lumière directe ou polarisée, réfléchie sous toutes les incidences par les corps transparens, mais encore à déterminer d'avance la proportion de lumière polarisée, si ce sont des rayons directs qu'on reçoit sur le corps transparent, ou la déviation du plan de polarisation, si l'on fait réfléchir de la lumière polarisée dans un

azimut oblique. Ainsi ces seules formules servent à calculer les lois de tous les phénomènes que présentent la réflexion partielle et totale à la première et à la seconde surface des corps transparens.

On doit publier incessamment dans le 'Bulletin des Sciences de la Société Philomatique,' un extrait de mon nouveau mémoire.

. . . . .

Tous ces mémoires, que dernièrement j'ai présentés coup sur coup à l'Académie des Sciences, ne m'en ont pas cependant ouvert la porte. C'est M. Dulong qui a été nommé pour remplir la place vacante dans la section de physique par l'élection de M. Fourier à celle de secrétaire perpétuel. Sans doute M. Dulong est un physicien de beaucoup de mérite; mais il est aussi habile chimiste, et pouvait entrer dans la section de chimie, si l'Académie avait eu grande envie de me recevoir. Je n'ai pas même été présenté sur le même rang que M. Dulong par les membres de la section de physique: ils l'ont mis le premier et moi le second. Vous voyez, Monsieur, que la théorie des ondulations ne m'a point porté bonheur; mais cela ne m'en dégoûte pas; et je me console de ce malheur en m'occupant d'optique avec une nouvelle ardeur.

Agréez, Monsieur, l'expression de ma haute considération, et présentez, je vous prie, mes hommages à Madame Young.

A. FRESNEL.

19.—*From* M. FRESNEL *to* Dr. YOUNG.

MONSIEUR, 27 Mars, 1823.

J'AI l'honneur de vous adresser sept exemplaires d'un extrait du mémoire que je vous avais annoncé dans ma dernière lettre, et qui a pour objet la recherche théorique et expérimentale des lois suivant lesquelles la lumière polarisée est modifiée par sa réflexion totale dans l'intérieur des corps transparens; je vous prie de vouloir bien accepter un de ces exemplaires, d'en offrir un de ma part à la Société Royale, et de remettre, ou faire parvenir, les autres à MM. Wollaston, Dalton, Herschel, Brewster et Leslie.

M. Brewster sera peut-être surpris qu'en publiant cet extrait, je n'y aie pas fait mention de ses recherches sur les effets de la

réflexion totale, qui sont antérieures aux miennes : la raison de mon silence à cet égard tient d'abord au peu d'espace dans lequel j'étais forcé de resserrer cet extrait ; et ensuite à la persuasion où je suis que M. Brewster s'est complettement mépris dans les lois qu'il a données des phénomènes de coloration que présente la lumière polarisée après avoir éprouvé la réflexion totale. D'abord il n'a pas observé que ces couleurs ne sont sensibles que dans les incidences voisines de la limite de la réflexion partielle ; ce qui fait soupçonner que le verre dont il se servait n'était pas bien recuit ; en second lieu, il a avancé que ces couleurs, qu'il suppose pareilles à celles des lames cristallisées, descendaient dans l'ordre des anneaux par deux, trois, quatre réflexions, &c., comme la teinte d'une lame cristallisée dont on double, triple, quadruple l'épaisseur ; tandis que dès l'incidence de 48°, par exemple, deux nouvelles réflexions détruisent presque entièrement l'effet produit par les deux premières et ramènent sensiblement la lumière à son état primitif de polarisation complette.

J'ignore au reste si le Dr. Brewster s'est occupé depuis des mêmes phénomènes : je ne connais que le mémoire qu'il a publié sur ce sujet dans les 'Transactions Philosophiques' de 1816 ou 1817 ; mémoire que M. Arago me montra lorsque je lui communiquai, en 1817, mes premières observations sur la dépolarisation produite par la réflexion totale.

Agréez, Monsieur, l'assurance de mon dévouement, et de la haute considération avec laquelle j'ai l'honneur d'être

Votre très-humble et obéissant serviteur,

A. FRESNEL.

20.—*From* M. FRESNEL *to* Dr. YOUNG.

MONSIEUR, Paris, le 16 Septembre, 1823.

EN vous écrivant après un silence aussi long j'aurais désiré pouvoir vous communiquer quelques nouvelles observations d'optique : malheureusement, depuis assez long temps j'ai été constamment occupé d'affaires de service et de détails relatifs à l'éclairage des phares. J'ai passé presque tout le mois de Juillet dans la tour de Cordouan, à l'embouchure de la

Gironde, pour y installer un appareil lenticulaire ou dioptrique, à feux tournans. N'ayant guères avec moi que de mauvais ouvriers, j'ai été obligé d'entrer dans les plus minutieux détails de cette installation, et de faire souvent moi-même l'ouvrier. La vivacité des éclats que présente le nouvel appareil a surpris les marins. Quelques Anglais que la saison des bains avait amenés à Royan, ont dit qu'ils n'avaient pas vu de phare aussi brillant sur les côtes d'Angleterre. Je désirerais savoir ce qu'en pensent vos marins, qui sont les plus expérimentés de l'Europe ; et s'ils trouvent que la durée de chaque apparition est suffisante pour relever le phare à la mer, comme l'estiment plusieurs marins français que j'ai consultés sur ce sujet. Cet appareil n'étant établi dans le phare de Cordouan que depuis le 25 Juillet dernier, ce ne sera sans doute que dans un ou deux mois d'ici que vous pourrez recueillir quelques observations de vos marins sur le nouveau feu de Cordouan. Si vous avez la bonté de me les communiquer, vous me rendrez un grand service. Le phare de Cordouan devant servir à guider les bâtimens qui entrent dans la Gironde, comme ceux qui passent au large, j'ai tâché de procurer aux premiers les avantages d'une lumière fixe, qui a quatre lieues marines de portée, et qui empêche de perdre le phare de vue pendant les éclipses du feu tournant, lorsqu'on approche des écueils dont l'embouchure de la Gironde est semée. Ce petit feu fixe est produit sans addition de lampes, sans augmentation dans la dépense d'huile, et seulement en recueillant les rayons qui passent par dessous l'appareil tournant et les réfléchissant vers l'horison par de petites glaces étamées disposées comme les feuilles d'une jalousie. Il n'est aucun phare, je crois, dans lequel on tire autant parti de la quantité d'huile employée. La consommation actuelle est d'une livre et demie d'huile par heure, au plus ; tandis que celle de l'ancien feu était de trois livres ; en sorte qu'il résulte à la fois de ce changement d'appareil, une économie annuelle de près de six mille francs et une grande augmentation de lumière.

J'ai l'honneur d'être, avec la plus haute considération,

Monsieur,

Votre très-humble et très-obéissant serviteur,

FRESNEL.

21.—*From* M. FRESNEL *to* Dr. YOUNG.

MONSIEUR, Paris, le 16 Octobre, 1824.

JE regrette beaucoup de ne pouvoir répondre en ce moment à la demande obligeante que vous me faites:* je suis occupé du matin au soir par des examens que je fais à l'Ecole Polytechnique, et je ne serai debarrassé que dans quinze jours de cette pénible occupation, qui m'a presque rendu malade. Il y a long-temps que je n'ai rien fait de neuf en optique. Je crois vous avoir envoyé, Monsieur, des extraits de mes deux derniers mémoires, sur la double réfraction singulière que la lumière subit en traversant le cristal de roche parallèlement à son axe, et sur la loi des modifications que la réflexion totale, dans les corps diaphanes, imprime à la lumière polarisée. Mais je ne vous ai pas encore communiqué l'extrait de mon mémoire sur la double réfraction, publié dans le Bulletin des Sciences de la Société Philomatique, livraison des mois d'Avril et de Mai 1822, parce que je n'en ai pas fait tirer d'exemplaires. Si vous n'avez pas le Bulletin de la Société Philomatique, et que vous désiriez lire ce court extrait d'un long mémoire, j'en ferai faire une copie, que j'aurai l'honneur de vous envoyer par la voie que vous voudrez bien m'indiquer.

J'ai maintenant, et depuis quelques années, des idées théoriques assez arrêtées sur les principaux phénomènes de l'optique, et je pourrais faire un article bien nourri en présentant ces vues dans un cadre resserré; mais ce n'est que dans quinze jours que je pourrais commencer à m'en occuper, et vous ne recevriez mon article que dans un mois. Quant à l'histoire de la science, personne n'est moins capable que moi de fournir des renseignemens, n'ayant pas l'avantage de pouvoir entendre les ouvrages et les journaux scientifiques écrits en Anglais, et n'ayant même que le tems de lire tout ce qui se publie en France sur l'optique.

* This was in answer to an application, made through Dr. Young, by the proprietors of the Encyclopædia Britannica, for an article on *Light*, for their new Supplement.—*Note by the Editor.*

Je vous prie de m'excuser mon brouillon ; je suis accablé par la fatigue et le besoin de sommeil.

J'ai l'honneur d'être, avec la plus haute considération,

Monsieur,

Votre très-humble et très-obéissant serviteur,

A. FRESNEL.

22.—*From* M. FRESNEL *to* Dr. YOUNG.

Paris, le 26 Novembre, 1824.

Rue des Fossés St. Victor, No. 19.

MONSIEUR,

SI j'ai tardé quelques jours à répondre à votre lettre du 17, c'est qu'une indisposition assez grave m'interdisait la plus légère occupation. Je n'ai encore en ce moment que le degré de force qui suffit pour écrire une lettre. Cette indisposition provient principalement de la fatigue de mes examens, et peut-être aussi du petit travail auquel je me suis livré en rédigeant un article pour la Revue Européenne, comme je m'y étais engagé. Cette leçon sévère m'avertit assez que je suis trop faible pour multiplier mes engagemens, et que ma santé exige absolument un répos de quelques mois. C'est avec regret que je me vois dans l'impossibilité d'écrire l'exposé de mes idées théoriques que vous me demandez.

En y réfléchissant bien, cependant, dois-je regretter de ne pouvoir travailler pour un ouvrage anglais ? Avons-nous bien de nous louer en France des jugemens qu'on porte en Angleterre de nos travaux et de nos découvertes ? Le Dr. Brewster prétend que c'est d'après ses idées qu'on a perfectionné l'éclairage du phare de Cordouan, quoique l'invention et l'exécution des lentilles à échelons soient toutes françaises, du commencement jusqu'à la fin. Il réclame aussi la découverte des modifications imprimées par la réflexion totale à la lumière polarisée ; modifications dont il n'avait pas une idée bien juste, si j'en juge par ce qu'il a publié sur ce sujet. D'après ce que m'a dit M. Arago, il paraît qu'on a fait très peu d'attention en Angleterre à la loi générale de la double réfraction, ainsi qu'aux formules que j'ai données pour calculer les intensités de la lumière réfléchie obliquement sur les corps transparens, et les déviations

du plan de polarisation. Ces formules m'ont fait découvrir la loi assez compliquée des modifications singulières que la réflexion totale en dedans des milieux diaphanes imprime à la lumière polarisée ; mais il ne paraît pas qu'on ait fait plus de cas chez vous de cette découverte que de celle de la double réfraction spéciale des rayons qui traversent le cristal de roche parallèlement à son axe. Si je parvenais à démontrer à M. Herschel, à M. Wollaston, et aux autres physiciens anglais, encore attachés au système de Newton, que la théorie des ondes mérite la préférence, ils ne manqueraient pas de dire que c'est uniquement à vos travaux qu'on doit le renversement du système de l'émission et les progrès de la théorie des ondes. Si, désabusant vos savans sur la polarisation mobile, je leur fesais adopter l'explication que j'ai donnée de la coloration des lames cristallisées et ces méthodes générales au moyen desquelles on peut calculer les teintes dans tous les cristaux quand on connaît la double réfraction de chaque espèce de rayon, ils diraient encore que l'explication de ces phénomènes vous appartient : ils vous attribueraient également celle des phénomènes compliqués de la diffraction.

Il me semble cependant (je ne sais si mon amour propre m'aveugle) que ce que vous m'aviez laissé à faire sur ces diverses parties de l'optique était aussi difficile que ce que vous aviez fait. Vous aviez cueilli les fleurs, pourrais-je dire avec la modestie anglaise, et j'ai creusé péniblement pour découvrir les racines.

Je suis loin de prétendre à ce qui vous appartient, Monsieur, comme vous l'avez vu dans le petit traité sur la lumière inséré dans le supplément à la traduction française de la Chimie de Thomson, comme vous le verrez encore dans l'article que je viens de rédiger pour la 'Revue Européenne.' J'ai avoué d'assez bonne grace devant le public, en plusieurs occasions, l'antériorité de vos découvertes, de vos observations, et même de vos hypothèses. Cependant, entre nous, je ne suis pas persuadé de la justesse de ce mot spirituel par lequel vous vous compariez à un arbre, et moi à une pomme que cet arbre aurait produite : j'ai la conviction intérieure que la pomme aurait poussé sans l'arbre, car les premières explications que je me suis données des phénomènes de la diffraction et des anneaux colorés, des lois de la

réflexion et de la réfraction, je les ai tirées de mon propre fonds, sans avoir lu votre ouvrage ni celui de Huyghens. J'ai remarqué aussi de moi-même que la différence de marche des rayons ordinaires et extraordinaires au sortir d'une lame cristallisée était égale à celle des rayons réfléchis à la première et à la seconde surface de la lame d'air qui donne la même teinte dans les anneaux colorés. C'est lorsque je communiquai cette observation à M. Arago qu'il me parla pour la première fois de la note que vous aviez publiée deux ans auparavant sur le même sujet, et à laquelle jusqu'alors il n'avoit pas compris grande chose. Au reste, ceci ne me donne pas le droit de partager avec vous, Monsieur, le mérite de ces découvertes, qui vous appartient exclusivement par la priorité : aussi, ai-je jugé inutile d'informer le public de tout ce que j'avais trouvé de mon côté, mais après vous ; et si je vous en parle, c'est uniquement pour justifier ma proposition paradoxale, *que la pomme serait venue sans l'arbre.* Il y a longtems, Monsieur, que je désirais vous parler sur ces sujets à cœur ouvert, et vous montrer naïvement toute l'étendue de mes prétentions.

Admettons que mon amour propre soit trop exigeant, et qu'on m'ait assez rendu justice dans votre pays (car je suis peut-être effectivement un des Français qui ont le moins à se plaindre de vos compatriotes), je n'en serais pas moins étonné, je dirais presque révolté, de ce qu'on me rapporte si souvent sur la partialité choquante avec laquelle vos journaux scientifiques élèvent tous les jours au-dessus des découvertes françaises les plus remarquables, ce qu'on a fait en Angleterre de plus insignifiant. Certes, je suis loin de disconvenir que vous n'ayez sur nous, surtout en politique, des supériorités incontestables ; mais vous avouerez au moins que nous l'emportons de beaucoup en impartialité et en amour de la justice.

Cette lettre vous paraîtra peut-être, Monsieur, la boutade d'un malade tourmenté par la bile, et dont l'amour propre est mécontent du peu d'attention qu'on a fait à ses travaux dans votre pays. Je suis loin de nier le prix que j'attacherais aux éloges des savans anglais, et de prétendre qu'ils ne m'auraient pas flatté agréablement. Mais, depuis longtems cette sensibilité, ou cette vanité qu'on appelle amour de la gloire, s'est beaucoup

émoussée en moi : je travaille bien moins pour capter les suffrages du public que pour obtenir une approbation intérieure qui a toujours été la plus douce récompense de mes efforts. Sans doute j'ai eu souvent besoin de l'aiguillon de la vanité pour m'exciter à poursuivre mes recherches dans les momens de dégoût ou de découragement ; mais tous les complimens que j'ai pu recevoir de MM. Arago, de Laplace, ou Biot, ne m'ont jamais fait autant de plaisir que la découverte d'une vérité théorique et la confirmation de mes calcules par l'expérience. Le peu d'empressement que j'ai mis à publier mes mémoires, dont il n'a guères paru que des extraits, montre que je ne suis pas tourmenté de la soif de la renommée, et que j'ai assez de philosophie pour ne pas attacher trop d'importance aux jouissances de la vanité. Mais il est inutile de m'étendre davantage sur ce sujet en écrivant à un homme trop supérieur pour que cette philosophie lui soit étrangère, et qui me comprendra et me croira aisément.

Agréez, Monsieur, l'assurance de la haute considération avec laquelle j'ai l'honneur d'être

Votre très-humble et très-obéissant serviteur,

FRESNEL.

P.S. Je ne parlerai point à M. Arago de votre seconde lettre. Je lui avais dit un mot de la première ; il a été surpris que vous me témoignassiez le désir d'avoir un exposé de mes idées théoriques sur la lumière pour un ouvrage où vous lui avez recommandé de ne rien mettre qui sentît l'hypothèse. Il part dans peu de jours pour Metz, où il espère terminer dans ses soirées son article sur la polarisation, par la description des modifications que la réflexion totale imprime à la lumière polarisée et des caractères singuliers de la polarisation circulaire.

23.—*From* M. FRESNEL *to* Dr. YOUNG.

MONSIEUR, Paris, le 19 Janvier, 1825.

LORSQUE je vous ai écrit ma dernière lettre, mon imagination était fatiguée par des idées qui revenaient sans cesse à

ma pensée, comme cela arrive souvent aux malades, et c'était pour m'en débarrasser, que je les mettais sur le papier. Mais j'aurais dû me borner à cela, et ne pas vous envoyer cette lettre qui a dû vous paraître assez ridicule et que je vous prie de jeter au feu.

La peine que vous avez prise de transcrire les complimens que vous m'avez adressés dans la préface de votre bel ouvrage sur les hiéroglyphes, me fait craindre que vous n'ayez pensé que mon amour-propre avait besoin de cette consolation. La vérité est que je n'éprouvais ni chagrin d'amour-propre ni sentiment d'aigreur en écrivant cette lettre, qui, je l'avoue, n'a pas dû vous en paraître exempte : je jetais sur le papier des idées qui fatiguaient mon imagination.

J'ai beaucoup tardé à vous répondre, Monsieur, et vous avez pu prendre mon silence pour un refus. J'ai toujours été languissant jusqu'à présent, et je ne suis pas encore guéri. On m'a recommandé d'éviter soigneusement toute tension d'esprit. Il est résulté de ce long repos que je me trouve très arriéré dans mes occupations *obligées;* en sorte que, lorsque je me sens capable de travailler un peu, c'est à elles que je dois consacrer de préférence ces courts momens. J'ai cependant commencé à rédiger une exposition de mes idées théoriques sur la polarisation de la lumière et les lois des interférences des rayons polarisés : j'espère que cette note sera terminée dans une dixaine de jours. J'attendais toujours pour vous répondre que je l'eusse commencée, espérant m'y mettre d'un jour à l'autre ; voilà pourquoi, Monsieur, j'ai tant tardé à vous écrire.

Il est très possible que vous n'ayez plus besoin maintenant de ce petit mémoire ; s'il vous était inutile, je vous prie d'avoir la bonté de m'en prévenir, afin que je ne vous fasse pas payer mal à propos un assez fort port de lettre.

Vous avez pu trouver, Monsieur, dans le tome XVII. des Annales de Chimie et de Physique, page 179 et suivantes, et dans les divers extraits de mes mémoires que j'ai eu l'honneur de vous envoyer ou de vous indiquer, un aperçu de mes travaux et de mes idées théoriques sur la polarisation et la double réfraction. La note que je me propose de vous envoyer contiendra seulement la démonstration rigoureuse des vibrations

transversales des rayons polarisés, et l'explication théorique des lois de l'interférence de ces rayons, sur lesquelles reposent tous mes calcules relatifs à la coloration des lames cristallisées; développemens que je n'avais pu donner, faute d'espace, dans l'article des Annales que je viens de citer. Apropos de cette théorie, il me semble que je puis en réclamer la seconde moitié. Vous avez remarqué et démontré le premier que les couleurs des lames cristallisées provenaient de la différence de marche des rayons ordinaires et extraordinaires; mais il restait à établir le sens de polarisation de ces rayons dans les lames minces; il fallait expliquer pourquoi leurs interférences ne produisaient des couleurs que lorsqu'on analysait la lumière émergente avec un rhomboïde de spath calcaire, ou par tout autre mode de polarisation; et pourquoi il était encore nécessaire que la lumière eût reçu une polarisation préalable avant de traverser la lame cristallisée. Je crois aussi être le premier qui ait donné des méthodes sûres et générales pour calculer les teintes que la polarisation développe dans les lames cristallisées.

Excusez, Monsieur, la brusque franchise de cette réclamation, qui m'engage à vous faire un article de votre dernière lettre où vous me dites: "Je crois avoir expliqué les phénomènes desquels M. Biot avait tiré cette notice imparfaite, avant que vous eussiez publié la même théorie, &c."

Agréez, Monsieur, l'assurance de la haute considération avec laquelle j'ai l'honneur d'être

Votre très-humble et très-obéissant serviteur,

FRESNEL.

24.—*From* Dr. YOUNG *to* M. ARAGO.

MY DEAR SIR, London, 25th January, 1825.

AT last I have to congratulate you on the cessation of your persecutions; but I must tell you in the same breath, that they are not quite at an end, and that you must positively send me what you have written; 1. on metallic mirrors; 2. on rings in plates perpendicular to the axis; and 3. on the absorption of light. I must protest against your having with-

held them, because *I thought* M. Biot's theory too foolish to merit any serious attention: you may believe that I did not mean to hint that your confutation of it was superfluous, so far as it might have acquired any credit with the public: nor did I say, or think, that the very refined and elegant experiments of our friend Fresnel made the paper too long. I can only say, that I hope to receive the three sections as they are written by return of post: in *that case* only can I answer for their being inserted, as I have told Mr. Napier that I *have received* your last words, and I shall send him the translation in a reasonable time. But I shall certainly not attempt to supply the deficiency by anything of my own; so that if you think the article imperfect for want of them, you must be responsible for your own omission. I shall only add a few theoretical conjectures, quite independent of the series of your sections. By his "*countrymen*," —— must certainly have meant *Scotchmen:* he could not have meant to assert that the 'Moniteur' had not been received in London at the time in question. Apropos of priority of observations, pray have the goodness to point out to Mr. Humboldt, in the 'Bulletin Universel des Sciences,' VII. 91, p. 177 *seq.*, this remarkable passage of Mr. Champollion Figeac, "Les signes 10 et 1 sont tels que M. JOMARD les a *publiés* en 1819; les chiffres hiératiques ont été déterminé par mon frère."

Perhaps Mr. Ch. will say with —— that he had not seen the 'Moniteur' in which Mr. Humboldt stated the history of Mr. Jomard's *publication*.* You will receive this, I hope, on Thursday, so that I may get an answer by Friday's post if possible; if you cannot send your answer till Monday, still send it and I will wait for it if I can. Adieu, mon cher confrère.

Always yours,
THOMAS YOUNG.

* See Vol. III. p. 208, of this work.

25.—*From* M. FRESNEL *to* Dr. YOUNG.

MONSIEUR, Paris, le 4 Septembre, 1825.

LORSQUE vous me demandâtes, il y a environ un an, de vous faire part de mes vues théoriques sur la polarisation et la double réfraction, j'eus l'honneur de vous indiquer l'extrait de mon mémoire sur la double réfraction qui avait été publié dans les deux bulletins de la Société Philomatique des mois d'Avril et Mai, 1822 : n'en ayant pas fait tirer d'exemplaires à part, je ne pouvais pas vous en envoyer. Vous me répondîtes que d'après l'indication que je vous donnais, vous comptiez le trouver aisément ; voilà pourquoi je ne crus pas nécessaire de vous en faire une copie.

Je regrette de n'avoir point encore trouvé le tems ni l'occasion de faire imprimer le mémoire en entier : ce que vous avez la bonté de me dire sur l'extrait me fait penser que la lecture du mémoire vous aurait offert quelque intérêt.

J'ai eu l'honneur de vous envoyer cet hiver, avec un exemplaire de cet extrait, un petit mémoire contenant des vues théoriques sur la polarisation de la lumière, dans lequel vous avez pu remarquer une démonstration assez méthodique de l'existence *exclusive* des vibrations transversales, si toutefois vous avez eu le tems de le lire. C'était précisément ces réflexions et ces développemens que je me proposais de vous communiquer pour l'Encyclopédie Britannique, mais ils sont arrivés trop tard : peut-être même ne les avez vous pas reçus. Ce petit mémoire avait été inséré dans le Bulletin des Sciences de la Société Philomatique du mois d'Octobre, 1824, publié, je crois, vers le milieu l'hiver.

Autant que je puis me rappeler, j'ai adréssé successivement mes deux paquets d'exemplaires à Sir Humfrey Davy ; c'est M. le Lieutenant-Colonel Wright qui a bien voulu se charger de les lui faire passer par le courrier de l'Ambassade. Comme votre lettre me fait supposer que vous ne les avez pas reçus, et que je crains qu'il n'en soit de même des autres savans auxquels je désirais aussi les offrir, je joins à cette lettre sept exemplaires de chaque espèce, en vous priant de les donner aux physiciens qu'ils pourraient intéresser.

Je vous prie, Monsieur, d'avoir la bonté d'offrir mes remercîmens particuliers aux membres de la Société Royale qui ont bien voulu lui faire valoir mes travaux. Je n'ai pas besoin de dire que c'est à vous que je crois être principalement redevable de la faveur qu'elle vient de m'accorder.

Agréez, Monsieur,
Les sentiments de mon respectueux attachement,
Votre dévoué serviteur,
FRESNEL.

P.S.—Je me porte mieux depuis plusieurs mois ; mais j'ai été toujours trop occupé pour me rétablir entièrement ; je compte faire incessamment un petit voyage, qui achèvera, j'espère, le rétablissement de ma santé.

26.—*From* Dr. YOUNG *to* M. ARAGO.

MY DEAR SIR, Park Square, 29th March, 1827.

IN sending you my annual contribution to the improvement of the 'Connaissance des Tems,' I have also the pride and pleasure to inform you that the Council of the Royal Society has done honour to us *all*, by awarding to our friend Fresnel the Rumford medal, which has been adjudged but once since the death of Malus. In this determination the most zealous supporter of the cause was Mr. Herschel : I was obliged to be silent, from being *too much* interested in the subject, but in fact there was no opposition. The value of the medal is 60*l.* ; there will be a sum of 50*l.* in money besides, which I shall have to remit, arising from the accumulations from the value of the medals not allotted. Thinking that this circumstance would make our system a little more popular than hitherto, I have determined to insert in my Astronomical and Nautical Collections, a translation of M. Fresnel's Abstract, which is published in 'Thomson's Chemistry,' and I trust he will not dislike its appearance.

Pray remember me kindly to Humboldt, who, I find from M. de Flavigny, is still at Paris, and tell him that I am still

going on, though slowly, with my Egyptian researches. It would be very satisfactory to our Orientalists, if the exact history of the vase with the name of "Xerxes"* could be ascertained, for some of them suspect it is a forgery, the writing being in the wrong direction, that is, from right to left, in the nail-headed character. Believe me always, my dear Sir, with best compliments to Madame Arago,

Very truly yours,
THOMAS YOUNG.

27.—*From* Dr. YOUNG *to* M. FRESNEL.

MY DEAR SIR, London, 9, Park Square, 18th June, 1827.

I HAVE great pleasure in transmitting to you the Prize Medal of Count Rumford, intended to be given biennially, to the author of the most important discovery or improvement relating to heat and light, which the Council of the Royal Society has thought it right to assign to your application of the undulatory theory of light to the phenomenon of polarisation. You will also have the goodness to call on Mr. Lafitte the banker, whom I have ordered to pay you the sum of 55*l.* 16*s.* sterling, and who will return me your receipt for the amount in French money: this sum being the accumulation derived from the investment of the value of medals not adjudged. At last, then, I trust you will no longer have to complain of the neglect which your experiments have for a time undergone in this country. I should also claim some right to participate in the compliment which is tacitly paid to myself in common with you by this adjudication, but considering that more than a quarter of a century is past since my principal experiments were made, I can only feel it a sort of anticipation of *posthumous* fame, which I have never particularly coveted.

Believe me, dear Sir, with great respect,
Very truly yours,
THOMAS YOUNG, M.D., *For. Sec.* R.S.

* See Vol. III. p. 248, of this work.

28.—*From* M. ARAGO *to* Dr. YOUNG.

MON CHER CONFRÈRE, Paris, 6 Août, 1827.

JE m'empresse de vous annoncer que l'Académie des Sciences, sur la proposition d'une commission dont j'étais membre, et qui m'avait confié l'honneur de développer vos titres, vient de vous nommer, à la place de Volta, l'un de ses *huit* associés étrangers. Vos concurrens étaient MM. Olbers, Bessel, Robert Brown, Blumenbach, Sœmmerring, Léopold de Buch, Dalton et Plana. Aussitôt que le Roi aura confirmé votre nomination, le secrétaire de l'Académie vous la notifiera officiellement.

Vous avez sans doute appris quelle perte cruelle les sciences ont faite le mois dernier. Le pauvre Fresnel était déjà à moitié éteint lorsque je lui remis vos médailles. Sa mort a plongé ici dans la plus vive douleur tous ceux qui sont dignes d'apprécier l'accord d'un beau talent et d'un beau caractère.

Adieu! mon cher confrère. Présentez, je vous prie, mes hommages respectueux à Madame Young, et agréez la nouvelle assurance de mon attachement.

Votre tout dévoué,

F. ARAGO.

29.—*From* Dr. YOUNG *to* M. ARAGO.

MY DEAR SIR, London, Park Square, 2nd September, 1827.

ON my return from Liverpool a few days ago, I found on my table your very obliging letter, announcing to me the success of your kind exertions in my favour, and my nomination as one of the eight foreign associates of the Academy. If any thing could add to the value of so distinguished a compliment, it would be the consciousness of owing it chiefly to the good opinion of so candid and so enlightened a judge as yourself. I must however confess that I could not read, without some confusion, my own name at the head of a list in which that of *Olbers* was only the third: but I am so much the more obliged to the Academy for its partiality to me.

I do indeed deeply lament the fatality which has a second time followed the adjudication of the Rumford medal. You do not tell me how far our poor friend felt that gratification from it, which it was our wish that he should receive, nor if he was pleased with my having undertaken to translate his Abstract into English. I have ordered the money which was due to him to be paid to his brother, whom the bankers represent as his natural heir: I suppose there can be no doubt that they are right.

Mrs. Young begs to unite with me in kind compliments and thanks: we are all occupied at present with the marriage of her sister, Miss Maxwell, which is to take place immediately, to a Mr. Earle.

Believe me, *cher confrère*,
Very truly yours,
THOMAS YOUNG.

No. XVIII.

THEORETICAL INVESTIGATIONS INTENDED TO ILLUSTRATE THE

PHENOMENA OF POLARISATION:

BEING AN ADDITION MADE BY DR. YOUNG TO M. ARAGO'S 'TREATISE ON THE POLARISATION OF LIGHT.'

From the Supplement to the Encyclopædia Britannica.

WRITTEN IN JANUARY, 1823.

WE are led from the facts which have been enumerated in the 11th section, respecting "uniaxal and biaxal crystals,"* to the remarkable coincidence between the discoveries of Dr. Brewster respecting crystals with two axes, and a theory which had been published a few years *earlier* in order to illustrate the propagation of an undulation in a medium compressed or dilated in a given direction only, and to prove that such an undulation must necessarily assume a spheroidal form upon the mechanical principles of the Huyghenian theory. As every contribution to the investigation of so difficult a subject may chance to be of some value, it will be worth while to copy this demonstration here, from the *Quarterly Review* for Nov. 1809, Vol. II. p. 345.

[Here follows the investigation given above in No. XII. pp. 230, 231, and 232.]

* This refers to a former section, added also by Dr. Young (by whom M. Arago's memoir was translated and edited), containing an enumeration of the principal minerals which possess one or two axes of double refraction, and referring more especially to Sir David Brewster's remarkable observations on the effects of heat and compression in producing or modifying double refraction. He found that compression was capable of producing colour when a soft animal jelly was only touched by the finger: and that when a negative crystal, like that of carbonate of lime, where the Huyghenian undulations producing the extraordinary ray are propagated in *oblate* spheroids, is compressed in the direction of the axis, the tints that it affords "descend," and that they "rise" when they are dilated: whence it seems to follow, that simple dilatation of a homogeneous substance in a given line will constitute that line the axis of an oblate spheroid. M. Fresnel succeeded in exhibiting not only colours by a strong pressure, but a very manifest reduplication of the image of a line, seen through a glass strongly compressed by screws.—*Note by the Editor.*

However satisfactorily such a mode of viewing the extraordinary refraction may be applied to the subsequent discoveries relating to the effects of heat and compression, there is another train of ideas, which arises more immediately from the phenomena of polarisation, and which might lead us to a more distinct notion of the separation of the pencil into two or more portions, though it does not seem to comprehend so entirely the phenomena depending on spheroidal undulations.

We may begin this mode of considering the subject in the words which have already been employed in the article CHROMATICS, *supra*, pp. 334, 335. "If we assume as a mathematical postulate, in the undulatory theory, without attempting to demonstrate its physical foundation, that a transverse motion may be propagated in a direct line, we may derive from this assumption a tolerable illustration of the subdivision of polarised light by reflection in an oblique plane. Supposing polarisation to depend on a transverse motion in the given plane; when a ray completely polarised is subjected to simple reflection in a different plane, at a surface which is destitute of any polarising action, and which may be said to afford a neutral reflection, the polar motion may be conceived to be reflected, as any other motion would be reflected at a perfectly smooth surface, the new plane of the motion being always the image of the former plane; and the effect of refraction will be nearly of a similar nature. But when the surface exhibits a new polarising influence, and the beams of light are divided by it into two portions, the intensity of each may be calculated, by supposing the polar motion to be resolved instead of being reflected, the simple velocities of the two portions being as the cosines of the angle, formed by the new planes of motion with the old, and the energies, which are the true measure of the intensity, as the squares of the sines. We are thus insensibly led to confound the intensity of the supposed polar motion with that of the light itself; since it was observed by Malus, that the relative intensity of the two portions, into which light is divided under such circumstances, is indicated by the proportion of the squares of the cosine and sine of the inclination of the planes of polarisation. The imaginary transverse motion must also ne-

cessarily be alternate, partly from the nature of a continuous medium, and partly from the observed fact, that there is no distinction between the polarisations produced by causes precisely opposed to each other in the same plane." Another analogous hint is found in the *Philosophical Transactions for* 1818, *supra*, p. 373 "Supposing the experiments to be perfectly represented by [Dr. Brewster's] general law, it will follow that the tint exhibited depends not on the difference of refracted densities in the direction of the ray transmitted, but on the greatest difference of refractive densities in directions perpendicular to that of the ray. These two conditions lead to the same result, where the effect of one axis only is considered, but they vary materially where two axes are supposed to be combined . . . There can be little doubt that the direction of the polarisation, in such cases, must be determined by that of the greatest and least of the refractive densities in question;" a "supposition," which Dr. Brewster finds "quite correct."

We may add again to these hints the consideration, that when simple pressure or extension in the direction of any given axis produces a spheroidal undulation in a medium before homogeneous, this state is always accompanied by the condition, that a ray describing the axis, while the densities in all transverse directions remain equal, undergoes no subdivision, but that a ray moving in the plane of the equator, to which the perpendiculars are the axis and another equatorial diameter, undergoes the greatest possible separation into parts that are respectively polarised in the planes passing through these directions.

From these phenomena we are led to be strongly impressed with the analogy of the properties of sound, as investigated cursorily by Mr. Wheatstone, and in a more elaborate manner by the multiplied experiments of Mr. Savart, which have shown that, in many cases, the elementary motions of the substances transmitting sound are transverse to the direction in which the sound is propagated, and that they remain in general parallel to the original impulse.

The next transition carries us from the *mathematical postulate* here mentioned to the *physical condition* assumed by Mr. Fresnel, that the relative situation of the particles of the etherial

medium with respect to each other, is such as to produce an elastic force tending to bring back a line of particles, which has been displaced, towards its original situation by the resistance of the particles *surrounding the line*, and at the same time to impel these particles in its own direction, and in that direction only, or principally, while the aggregate effect is propagated in concentric surfaces.

This hypothesis of Mr. Fresnel is at least very ingenious, and may lead us to some satisfactory computations: but it is attended by one circumstance which is perfectly *appalling* in its consequences. The substances on which Mr. Savart made his experiments were *solids* only; and it is only to solids that such a *lateral* resistance has ever been attributed: so that if we adopted the distinctions laid down by the reviver of the undulatory system himself, in his *Lectures*, it might be inferred that the luminiferous ether, pervading all space, and penetrating almost all substances, is not only highly elastic, but absolutely solid!!! The passage in question is this: (Vol. I. p. 627.)

"The immediate cause of solidity, as distinguished from liquidity, is the lateral adhesion of the particles to each other, to which the degree of hardness or solidity is always proportional. This adhesion prevents any change of the relative situation of the particles, so that they cannot be withdrawn from their places without experiencing a considerable resistance from the force of cohesion, while those of liquids may remain equally in contact with the neighbouring particles, notwithstanding their change of form. When a perfect solid is extended or compressed, the particles, being retained in their situations by the force of lateral adhesion, can only approach directly to each other, or be withdrawn further from each other; and the resistance is nearly the same, as if the same substance, in a fluid state, were enclosed in an unalterable vessel, and forcibly compressed or dilated. Thus the resistance of ice to extension or compression is found by experiment to differ very little from that of water contained in a vessel; and the same effect may be produced even when the solidity is not the most perfect that the substance admits; for *the immediate resistance of iron or steel to flexure is the same, whether it may be harder or softer.* It often happens,

however, that the magnitude of the lateral adhesion is so much limited, as to allow a capability of extension or compression, and it may yet retain a power of restoring the bodies to their original form by its reaction. This force may even be the principal or the only source of the body's elasticity: thus when a piece of elastic gum is extended, the mean distance of the particles is not materially increased . . and the change of form is rather to be attributed to a displacement of the particles than to their separation to a greater distance from each other, and the resistance must be derived from the lateral adhesion only: some other substances also, approaching more nearly to the nature of liquids, may be extended to many times their original length, with a resistance continually increasing; and in such cases there can scarcely be any material changes of the specific gravity of these substances. Professor Robison has mentioned the juice of bryony as affording a remarkable instance of such viscidity.

"It is probable that the immediate cause of the lateral adhesion of solids is a symmetrical arrangement of their constituent parts; it is certain that almost all bodies are disposed, in becoming solid, to assume the form of crystals, which evidently indicates the existence of such an arrangement; and all the hardest bodies in nature are of a crystalline form. It appears, therefore, consistent both with reason and with experience to suppose, that a crystallization more or less perfect is the universal cause of solidity. We may imagine, that when the particles of matter are disposed without any order, they can afford no strong resistance to a motion in any direction; but when they are regularly placed in certain situations with respect to each other, any change of form must displace them in such a manner, as to increase the distance of a whole rank at once; and hence they may be enabled to co-operate in resisting such a change. Any inequality of tension in a particular part of a solid, is also probably so far the cause of hardness, as it tends to increase the strength of union of any part of a series of particles which must be displaced by a change of form."

It must, however, be admitted, that this passage by no means contains a demonstration of the total incapability of fluids to transmit any impressions by lateral adhesion, and the hypothe-

sis remains completely open for discussion, notwithstanding the apparent difficulties attending it; which have appeared to bring us very near to the case stated in the same lectures as a possible one, that there may be independent worlds, some existing in different parts of space, others *pervading each other unseen and unknown in the same space.** We may perhaps accommodate the hypothesis of Mr. Fresnel to the phenomena of the ordinary and extraordinary refraction, by considering the undulations as propagated through the given medium in two different ways; some by the divergence of the elementary motions in the direction of the ray, and others by their remaining parallel to the direction of the impulse or of the polarisation: the former must be supposed to furnish the spheroidal, the latter the spherical refraction. It would indeed follow that the velocity of the spherical undulation ought to vary by innumerable degrees, within certain limits, according to the direction of the supposed elementary motion: while in fact the actual velocity of the spherical undulations seems to be uniformly equal to the velocity in the direction of the axis: but this objection may be obviated by supposing the surface so constituted, that for some unknown reason the parallel elementary motion can only be propagated in the regular manner when it takes place in the direction of the axis, or when it is made to assume that direction: a condition not very simple or natural, but by no means inconceivable; unless we saw any reason to consider the adhesion as a constant force, independent of the direction, and equal to the least or greatest elasticity, or unless it were possible to derive the phenomena of two supposed axes of polarisation, which Mr. Fresnel has explained on the hypothesis of two spheroids, from the supposition of two spherical undulations propagating oblique elementary motions in the direction of the actual polarisation as already determined for these crystals.

If these conjectures should be found to afford a single step, in an investigation so transcendently delicate, it will be best to pause on them for a time, and to wait for further aid from a new supply of experiments and observations.

* Vol. i., p. 610.

No. XIX.

AN ESSAY ON THE COHESION OF FLUIDS.

From the Philosophical Transactions for 1805.

Read December 20, 1804.

With some Alterations and Additions made by the Author to the Reprint of it in the Appendix to his Lectures on Natural Philosophy in 1807.

I.—General Principles.

It has already been asserted, by Mr. Monge and others, that the phenomena of capillary tubes are referable to the cohesive attraction of the superficial particles only of the fluids employed; and that the surfaces must consequently be formed into curves of the nature of lintearіae, which are supposed to be the results of a uniform tension of a surface, resisting the pressure of a fluid, either uniform, or varying according to a given law. Segner, who appears to have been the first that maintained a similar opinion, has shown in what manner the principle may be deduced from the doctrine of attraction, but his demonstration is complicated, and not perfectly satisfactory; and in applying the law to the forms of drops, he has neglected to consider the very material effects of the double curvature, which is evidently the cause of the want of a perfect coincidence of some of his experiments with his theory. Since the time of Segner, little has been done in investigating accurately and in detail the various consequences of the principle.

It will perhaps be most agreeable to the experimental philosopher, although less consistent with the strict course of logical argument, to proceed in the first place to the comparison of this theory with the phenomena, and to inquire afterwards for its

foundation in the ultimate properties of matter. But it is necessary to premise one observation, which appears to be new, and which is equally consistent with theory and with experiment; that is, that for each combination of a solid and a fluid, there is an appropriate angle of contact, between the surfaces of the fluid, exposed to the air, and to the solid. This angle, for glass and water, and in all cases where a solid is perfectly wetted by a fluid, is evanescent: for glass and mercury, it is about 140° in common temperatures, and when the mercury is moderately clean.

II.—Form of the Surface of a Fluid.

It is well known, and it results immediately from the composition of forces, that where a line is equally distended, the force that it exerts, in a direction perpendicular to its own, is directly as its curvature; and the same is true of a surface of simple curvature; but where the curvature is double, each curvature has its appropriate effect, and the joint force must be as the sum of the curvatures in any two perpendicular directions. For this sum is equal, whatever pair of perpendicular directions may be employed, as is easily shown by calculating the versed sines of two equal arcs taken at right angles in the surface. Now when the surface of a fluid is convex externally, its tension is produced by the pressure of the particles of the fluid within it, arising from their own weight, or from that of the surrounding fluid; but when the surface is concave, the tension is employed in counteracting the pressure of the atmosphere, or where the atmosphere is excluded, the equivalent pressure arising from the weight of the particles suspended from it by means of their cohesion, in the same manner as, when water is supported by the atmospheric pressure in an inverted vessel, the outside of the vessel sustains a hydrostatic pressure proportionate to the height; and this pressure must remain unaltered, when the water, having been sufficiently boiled, is made to retain its situation for a certain time by its cohesion only, in an exhausted receiver. When, therefore, the surface of the fluid is terminated by two right lines, and has only a simple curvature, the curvature must be everywhere as the ordinate; and where it has a

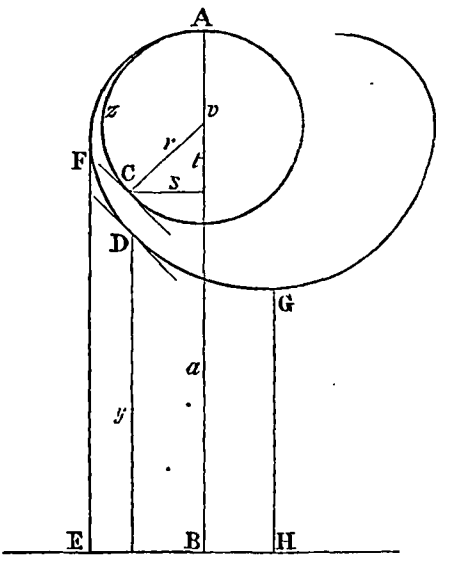
double curvature, the sum of the curvatures in the different directions must be as the ordinate. In the first case, the curve may be constructed by approximation, if we set out from a point at which it is either horizontal or vertical, and divide the height into a number of small portions, and taking the radius of each proportional to the reciprocal of the height of its middle point, above or below the general surface of the fluid, go on to add portions of circles joining each other, until they have completed as much of the curve as is required. In the second case it is only necessary to consider the curve derived from a circular basis, which is a solid of revolution; and the centre of that circle of curvature, which is perpendicular to the section formed by a plane passing through the axis, is in the axis itself, consequently in the point where the normal of the curve intersects the axis: we must therefore here make the sum of this curvature, and that of the generating curve, always proportional to the ordinate. This may be done mechanically, by beginning at the vertex, where the two curvatures are equal, then for each succeeding portion, finding the radius of curvature, by deducting the proper reciprocal of the normal, at the beginning of the portion, from the ordinate, and taking the reciprocal of the remainder. In this case the analysis leads to fluxional equations of the second order, which appear to afford no solution by means hitherto discovered; but the cases of simple curvature may be more easily subjected to calculation; the curvature varying always as the ordinate, the curve belongs to the general description of an elastic curve.

III.—Analysis of the simplest Forms.*

Let the greatest ordinate of the curve (AB) be called a, the arc of the circle of curvature at the vertex (AC) z, and let us suppose, that while this circle is uniformly increased, the curve (AD) flows with an equal angular velocity, then the fluxion of the curve, being directly as the radius

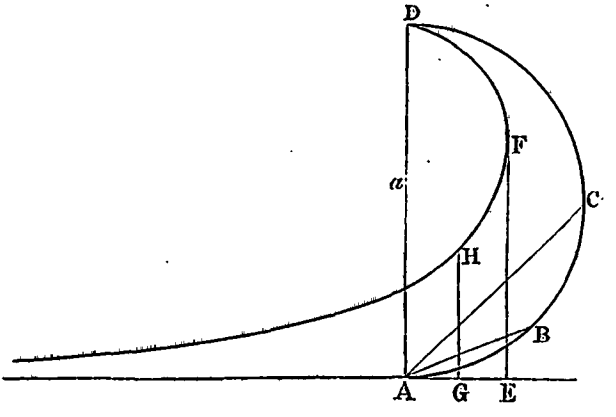
* In the original Essay, the mathematical form of this investigation and the figures were suppressed, the reasoning and the results to which it leads being expressed in ordinary language: even in its altered form the investigation is unduly concise and obscure.—*Note by the Editor.*

of curvature, will be inversely as the ordinate y, and will be expressed by $\frac{a\dot{z}}{y}$; the fluxion of the absciss will therefore be $\frac{ta\dot{z}}{ry}$, t being the cosine of the arc z, and r the radius, and the fluxion of the area will be $\frac{ta\dot{z}}{r}$. But $\frac{t\dot{z}}{r}$ is the fluxion of the sine s of the arc z in the circle to which it belongs; consequently, *the area* is expressed by as, and *is equal to the rectangle contained by the initial ordinate, and the sine corresponding to each point of the curve in the initial circle of curvature.* Hence it follows, that *the whole area* (ABEF or EFGH) *included by the ordinates where the curve is vertical and where it is horizontal, is equal to the rectangle contained by the ordinate and the radius of curvature.*



In order to find the ordinate y, corresponding to a given angular direction, and to a given arc z, we have $\mp \dot{y} = \frac{sa\dot{z}}{ry}$, or, since $\frac{s\dot{z}}{r}$ is the fluxion of the versed sine v, $\mp \dot{y} = \frac{a\dot{v}}{y}$, and $\overline{\mp}\, y\dot{y} = a\dot{v}$, whence $yy = b \mp 2av$. But at the summit of the curve, when $v = 0$, $y = a$, therefore $b = aa$, and $yy = aa - 2av$; and where the curve meets the absciss, $y = 0$ and $a = 2v$. If $a = 4r$, when $y = 0$, v will be $2r$, and the curve will touch the horizontal line at an infinite distance, since its curvature must be infinitely small; if a be greater than $4r$, the least ordinate will be $\sqrt{(aa - 4ar)}$. When the curve is vertical, $v = r$, and $yy = aa - 2ar$. The rectangle, contained by the elevation above the general surface, and the diameter of the circle of curvature, which is here $2ar$, is constant in all circumstances for the same fluid; and may therefore be called the

appropriate rectangle of the fluid; and when the curve is infinite, and $a = 4ar$, this rectangle is equal to $8rr$, or to $\frac{1}{2}aa$, so that r and a may be readily found from it: it is also equal to the square of the ordinate at the vertical point, where $yy = aa - 2ar$. If we describe a circle ABCD, of which the



diameter is a, the chord of the arc of this circle (AC, AB,) corresponding in angular situation to the curve, will be equal to the ordinate (EF, GH,) at the respective point; for the versed sine in this circle will be $2v$, and the chord will be a mean proportional between a and $a-2v$; in this case therefore, where the curve is infinite, the ordinate varies as the sine of half the angle of elevation.

For determining the abscis, it would be necessary to employ an infinite series; and the most convenient would perhaps be that which is given by Euler for the elastic curve, in the second part of the third volume of the Acta Petropolitana.

IV.—Application to the Elevation of particular Fluids.

The simplest phenomena, which afford us data for determining the fundamental properties of the superficial cohesion of fluids, are their elevation and depression between plates and in capillary tubes, and their adhesion to the surface of solids, which are raised, in a horizontal situation, to a certain height above the general surface of the fluids. When the distance of a pair of

plates, or the diameter of a tube, is very minute, the curvature may be considered as uniform, and the appropriate rectangle may readily be deduced from the elevation, recollecting that the curvature in a capillary tube is double, and the height therefore twice as great as between two plates. In the case of the elevation of a fluid, in contact with a horizontal surface, the ordinate may be determined from the weight required to produce a separation; and the appropriate rectangle may be found in this manner also, the angle of contact being properly considered, in this as well as in the former case. It will appear that these experiments by no means exhibit an immediate measure of the mutual attraction of the solid and fluid, as some authors have supposed.

Sir Isaac Newton asserts, in his Queries, that water ascends between two plates of glass at the distance of one hundredth of an inch, to the height of about one inch, the product of the distance and the height being about .01; but this appears to be much too little. In the best experiment of Musschenbroek, with a tube, half of the product was .0196; in several of Weitbrecht, apparently very accurate, .0214. In Monge's experiments on plates, the product was 2.6 or 2.7 lines, or about .0210. Mr. Attwood says that, for tubes, the product is .0530, half of which is .0265. Until more accurate experiments shall have been made, we may be contented to assume .02 for the rectangle appropriate to water, and .04 for the product of the height in a tube by its bore. Hence, when the curve becomes infinite, its greatest ordinate is .2, and the height of the vertical portion, or the height of ascent against a single vertical plane, .14, or nearly one-seventh of an inch.

Now when the horizontal surface of a solid is raised from a vessel of water, the surface of the water is formed into a linteariа, to which the solid is a tangent at its highest point, and if the solid be still further raised, the water will separate; the surface of the water, being horizontal at the point of contact, cannot add to the weight tending to depress the solid, which is therefore simply the hydrostatic pressure of a column of water equal in height to the elevation, in this case one-fifth of an inch, and standing on the given surface. The weight of such a

column will be 50½ grains for each square inch, and in Taylor's well-known experiment the weight required was 50 grains. But when the solid employed is small, the curvature of the horizontal section of the water, which is convex externally, will tend to counteract the vertical curvature and to diminish the height of separation; thus, if a disc of an inch in diameter were employed, the curvature in this direction would perhaps be equivalent to the pressure of about one-hundredth of an inch, and might reduce the height from 2 to about .19, and the weight in the same proportion. There is, however, as great a diversity in the results of different experiments on the force required to elevate a solid from the surface of a fluid, as in those of the experiments on capillary tubes; and indeed the sources of error appear to be here more numerous. Mr. Achard found that a disc of glass, 1½ inch French in diameter, required, at 69° Fahrenheit, a weight of 91 French grains to raise it from the surface of water; this is only 37 English grains for each square inch; at 44½° the force was $\frac{1}{14}$ greater, or 39½ grains, the difference being $\frac{1}{3}\frac{1}{4}\frac{1}{3}$ for each degree of Fahrenheit. It might be inferred, from these experiments, that the height of ascent in a tube of a given bore, which varies in the duplicate ratio of the height of adhesion, is diminished about $\frac{1}{180}$ for every degree of Fahrenheit that the temperature is raised above 50°; there was, however, probably some considerable source of error in Achard's experiments, for I find that this diminution does not exceed $\frac{1}{1000}$. The experiments of Mr. Dutour make the quantity of water raised equal to 44.1 grains for each square inch. Mr. Achard found the force of adhesion of sulfuric acid to glass, at 69° of Fahrenheit, 1.26, that of water being 1; hence the height was as .69 to 1, and its square as .47 to 1, which is the corresponding proportion for the ascent of the acid in a capillary tube, and which does not very materially differ from the proportion of .395 to 1, assigned by Barruel for this ascent. Musschenbroek found it .8 to 1, but his acid was probably weak. For alcohol the adhesion was as .593, the height as .715, and its square as .510: the observed proportion in a tube, according to an experiment of Musschenbroek, was about .550, according to Carré from .400 to .440. The expe-

riments on sulfuric ether do not agree quite so well, but its quality is liable to very considerable variations. Dutour found the adhesion of alcohol .58, that of water being 1.

With respect to mercury, it has been shown by Professor Casbois of Metz, and by others, that its depression in tubes of glass depends on the imperfection of the contact, and that when it has been boiled in the tube often enough to expel all foreign particles, the surface may even become concave instead of convex, and the depression be converted into an elevation. Perhaps this change may be the effect of the commencement of a chemical action between the mercury and the component parts of the glass; but in barometers constructed according to the usual methods, the angle of the mercury will be found to differ little from 140°; and in other experiments, when proper precautions are taken, the inclination will be nearly the same. The determination of this angle is necessary for finding the appropriate rectangle for the curvature of the surface of mercury, together with the observations of the quantity of depression in tubes of a given diameter. The table published by Mr. Cavendish, from the experiments of his father, Lord Charles Cavendish, appears to be best suited for this purpose. I have constructed a diagram, according to the principles already laid down, for each case, and I find that the rectangle which agrees best with the phenomena is .01. The mean depression is always .015, divided by the diameter of the tube: in tubes less than half an inch in diameter, the curve is very nearly elliptic; and the central depression in the tube of a barometer may also be found by deducting from the corresponding mean depression the square root of one thousandth part of its diameter. There is reason to suspect a slight inaccuracy towards the middle of Lord Charles Cavendish's Table, from a comparison with the calculated mean depression, as well as from the results of the mechanical construction. The ellipsis approaching nearest to the curve may be determined by the solution of a biquadratic equation.

Diameter in inches.	Grains in an inch. C.	Mean depression by calculation. Y.	Central depression by observation. C.	Central depression by formula. Y.	Central depression by diagram. Y.	Marginal depression by diagram. Y.
.6	972	.025	.005	(.001)	.005	.066
.5	675	.030	.007	.008	.007	.067
.4	432	.037	.015	.017	.012	.069
.35	331	.043	.025	.024	.017	.072
.30	243	.050	.036	.033	.027	.079
.25	169	.060	.050	.044	.038	.086
.20	108	.075	.067	.061	.056	.096
.15	61	.100	.092	.088	.085	.116
.10	27	.150	.140	.140	.140	.161

The square root of the rectangle .01, or .1, is the ordinate where the curve would become vertical if it were continued; but in order to find the height at which the mercury adheres to a vertical surface of glass, we must diminish this ordinate in the proportion of the sine of 25° to the sine of 45°, and it will become .06, for the actual depression in this case. The elevation of the mercury that adheres to the lower horizontal surface of a piece of glass, and the thickness at which a quantity of mercury will stand when spread out on glass, supposing the angle of contact still 140°, are found, by taking the proportion of the sines of 20° and of 70° to the sine of 45°, and are therefore .0484 and .1330 respectively. If, instead of glass, we employed any surface capable of being wetted by mercury, the height of elevation would be .141, and this is the limit of the thickness of a wide surface of mercury, supported by a substance wholly incapable of attracting it. Now the hydrostatic pressure of a column of mercury .0484 in thickness, on a disc of one inch diameter, would be 131 grains; to this the surrounding elevation of the fluid will add about 11 grains for each inch of the circumference, with some deduction for the effect of the contrary curvature of the horizontal section, tending to diminish the height; and the apparent cohesion thus exhibited will be about 160 grains, which is a little more than four times as great as the apparent cohesion of glass and water. With a disc 11 lines in diameter, Mr. Dutour found it 194 French grains, which is equivalent to 152 English grains, instead of 160 for an inch; a result which is sufficient to confirm the principles of the calculation. The depth of a quantity of mercury standing on glass I have found, by actual observation, to agree precisely with this calculation. Segner says that

the depth was .1358, both on glass and on paper; the difference is very trifling, but this measure is somewhat too great for glass and too small for paper, since it appears from Dutour's experiments that the attraction of paper to mercury is extremely weak.

If a disc of a substance capable of being wetted by mercury, an inch in diameter, were raised from its surface in a position perfectly horizontal, the apparent cohesion should be 381 grains, taking .141 as the height; and for a French circular inch, 433 grains, or 528 French grains. Now in the experiments of Morveau, the cohesion of a circular inch of gold to the surface of mercury appeared to be 446 grains, of silver 429, of tin 418, of lead 397, of bismuth 372, of zinc 204, of copper 142, of metallic antimony 126, of iron 115, of cobalt 8; and this order is the same with that in which the metals are most easily amalgamated with mercury. It is probable that such an amalgamation actually took place in some of the experiments, and affected their results; for the process of amalgamation may often be observed to begin almost at the instant of contact of silver with mercury, and the want of perfect horizontality appears in a slight degree to have affected them all. A deviation of one-fiftieth of an inch would be sufficient to have produced the difference between 446 grains and 528; and it is not impossible that all the differences, as far down as bismuth, may have been accidental. But if we suppose the gold only to have been perfectly wetted by the mercury, and all the other numbers to be in due proportions, we may find the appropriate angle for each substance by deducting from 180°, twice the angle, of which the sine is to the radius, as the apparent cohesion of each to 446 grains; that is, for gold 1, for silver about .97, for tin .95, for lead .90, for bismuth .85, for zinc .46, for copper .32, for antimony .29, for iron .26, and for cobalt .02, neglecting the surrounding elevation, which has less effect in proportion as the surface employed is larger. Gellert found the depression of melted lead in a tube of glass multiplied by the bore equal to about .054.

It would perhaps be possible to pursue these principles so far as to determine in many cases the circumstances under which a drop of any fluid would detach itself from a given surface.

But it is sufficient to infer, from the law of the superficial cohesion of fluids, that the linear dimensions of similar drops, depending from a horizontal surface, must vary precisely in the same ratio as the heights of ascent of the respective fluids against a vertical surface, or as the square roots of the heights of ascent in a given tube; hence the magnitudes of similar drops of different fluids must vary as the cubes of the square roots of the heights of ascent in a tube. I have measured the heights of ascent of water and of diluted spirit of wine in the same tube, and I found them nearly as 100 to 64: a drop of water falling from a large sphere of glass weighed 1.8 grains, a drop of the spirit of wine about .85, instead of .82, which is nearly the weight that would be inferred from the consideration of the heights of ascent, combined with that of the specific gravities. We may form a conjecture respecting the probable magnitude of a drop, by inquiring what must be the circumference of the fluid that would support by its cohesion the weight of a hemisphere depending from it: this must be the same as that of a tube, in which the fluid would rise to the height of one-third of its diameter; and the square of the diameter must be three times as great as the appropriate product, or, for water, .12; whence the diameter would be .35, or a little more than one-third of an inch, and the weight of the hemisphere would be 2.8 grains. If more water were added internally, the cohesion would be overcome, and the drop would no longer be suspended; but it is not easy to calculate what precise quantity of water would be separated with it. The form of a bubble of air rising in water is determined by the cohesion of the internal surface of the water, exactly in the same manner as the form of a drop of water in the air. The delay of a bubble of air at the bottom of a vessel appears to be occasioned by a deficiency of the pressure of the water between the air and the vessel; it is nearly analogous to the experiment of making a piece of wood remain immersed in water, when perfectly in contact with the bottom of the vessel containing it. This experiment succeeds, however, far more readily with mercury, since the capillary cohesion of the mercury prevents its insinuating itself under the wood.

V.—Of Apparent Attractions and Repulsions.

The apparent attraction of two floating bodies, round both of which the fluid is raised by cohesive attraction, is produced by the excess of the atmospheric pressure on the remote sides of the solids, above its pressure on their neighbouring sides: or, if the experiments are performed in a vacuum, by the equivalent hydrostatic pressure or suction, derived from the weight and the immediate cohesion of the intervening fluid. This force varies ultimately in the inverse ratio of the square of the distance; for if two plates approach each other, the height of the fluid, that rises between them, is increased in the simple inverse ratio of the distance; and the mean action, or negative pressure of the fluid, on each particle of the surface, is also increased in the same ratio. When the floating bodies are both surrounded by a depression, the same law prevails, and its demonstration is still more simple and obvious. The repulsion of a wet and a dry body does not appear to follow the same proportion: for it by no means approaches to infinity upon the supposition of perfect contact; its maximum is measured by half the sum of the elevation and depression on the remote sides of the substances, and as the distance increases, this maximum is only diminished by a quantity, which is initially as the square of the distance. The figures of the solids concerned modify also sometimes the law of attraction, so that, for bodies surrounded by a depression, there is sometimes a maximum, beyond which the force again diminishes; and it is hence that a light body floating on mercury, in a vessel little larger than itself, is held in a stable equilibrium without touching the sides. The reason of this will become apparent, when we examine the direction of the surface necessarily assumed, by the mercury, in order to preserve the appropriate angle of contact; the tension acting with less force, when the surface attaches itself to the angular termination of the float in a direction less horizontal.

The apparent attraction produced between solids, by the interposition of a fluid, does not depend on their being partially

immersed in it; on the contrary, its effects are still more powerfully exhibited in other situations; and, when the cohesion between two solids is increased and extended by the intervention of a drop of water or of oil, the superficial cohesion of these fluids is fully sufficient to explain the additional effect. When wholly immersed in water, the cohesion between two pieces of glass is little or not at all greater than when dry: but if a small portion only of a fluid be interposed, the curved surface, that it exposes to the air, will evidently be capable of resisting as great a force, as it would support from the pressure of the column of fluid, that it is capable of sustaining in a vertical situation; and in order to apply this force, we must employ, in the separation of the plates, as great a force as is equivalent to the pressure of a column, of the height appropriate to their distance. Morveau found that two discs of glass, 3 inches French in diameter, at the distance of one tenth of a line, appeared to cohere with a force of 4719 grains, which is equivalent to the pressure of a column 23 lines in height: hence the product of the height and the distance of the plates is 2.3 lines, instead of 2.65, which was the result of Monge's experiments on the actual ascent of water. The difference is much smaller than the difference of the various experiments on the ascent of fluids; and it may easily have arisen from a want of perfect parallelism in the plates; for there is no force tending to preserve this parallelism. The error, in the extreme case of the plates coming into contact at one point, may reduce the apparent cohesion to one half.

The same theory is sufficient to explain the law of the force, by which a drop is attracted towards the junction of two plates, inclined to each other, and which is found to vary in the inverse ratio of the square of the distance; whence it was inferred by Newton that the primitive force of cohesion varies in the simple inverse ratio of the distance, while other experiments lead us to suppose that cohesive forces in general vary in the direct ratio of the distance. But the difficulty is removed, and the whole of the effects are satisfactorily explained, by considering the state of the marginal surface of the drop. If the plates were parallel, the capillary action would be equal on both sides

of the drop: but when they are inclined, the curvature of the surface at the thinnest part requires a force proportioned to the appropriate height to counteract it: and this force is greater than that which acts on the opposite side. But if the two plates are inclined to the horizon, the deficiency may be made up by the hydrostatic weight of the drop itself; and the same inclination will serve for a larger or a smaller drop at the same place. Now when the drop approaches to the line of contact, the difference of the appropriate heights for a small drop of a given diameter will increase as the square of the distance decreases; for the fluxion of the reciprocal of any quantity varies inversely as the square of that quantity; and, in order to preserve the equilibrium, the sine of the angle of elevation of the two plates must be nearly in the inverse ratio of the square of the distance of the drop from the line of contact, as it actually appears to have been in Hauksbee's experiments.

VI.—Physical Foundation of the Law of Superficial Cohesion.

We have now examined the principal phenomena which are reducible to the simple theory of the action of the superficial particles of a fluid. We are next to investigate the natural foundations upon which that theory appears ultimately to rest. We may suppose the particles of liquids, and probably those of solids also, to possess that power of repulsion, which has been demonstratively shown by Newton to exist in aëriform fluids, and which varies in the simple inverse ratio of the distance of the particles from each other. In airs and vapours this force appears to act uncontrolled; but in liquids it is overcome by a cohesive force, while the particles still retain a power of moving freely in all directions; and in solids the same cohesion is accompanied by a stronger or weaker resistance to all lateral motion, which is perfectly independent of the cohesive force, and which must be cautiously distinguished from it. It is simplest to suppose the force of cohesion nearly or perfectly constant in its magnitude, throughout the minute distance to which it extends, and owing its apparent diversity to the

contrary action of the repulsive force, which varies with the distance. Now in the internal parts of a liquid these forces hold each other in a perfect equilibrium, the particles being brought so near, that the repulsion becomes precisely equal to the cohesive force that urges them together; but whenever there is a curved or angular surface, it may be found, by collecting the actions of the different particles, that the cohesion must necessarily prevail over the repulsion, and must urge the superficial parts inwards, with a force proportional to the curvature, and thus produce the effect of a uniform tension of the surface. For, if we consider the effect of any two particles in a curved line on a third at an equal distance beyond them, we shall find that the result of their equal attractive forces bisects the whole angle formed by the lines of direction; but that the result of their repulsive forces, one of which is twice as great as the other, divides it in the ratio of one to two, forming with the former result an angle equal to one sixth of the whole; so that the addition of a third force is necessary, in order to retain these two results in equilibrium; and this force must be in a constant ratio to the evanescent angle which is the measure of the curvature, the distance of the particles being constant. The same reasoning may be applied to all the particles which are within the influence of the cohesive force; and the conclusions are equally true if the cohesion is not precisely constant, but varies less rapidly than the repulsion.

VII.—Cohesive Attraction of Solids and Fluids.

When the attraction of the particles of a fluid for a solid is less than their attraction for each other, there will be an equilibrium of the superficial forces, if the surface of the fluid make with that of the solid a certain angle, the versed sine of which is to the diameter, as the mutual attraction of the fluid and solid particles is to the attraction of the particles of the fluid among each other. For, when the fluid is surrounded by a vacuum or by a gas, the cohesion of its superficial particles acts with full force in producing a pressure; but when it is any where in contact with a solid substance of the same attractive

power with itself, the effects of this action must be as much destroyed as if it were an internal portion of the fluid. Thus, if we imagined a cube of water to have one of its halves congealed, without any other alteration of its properties, it is evident that its form and the equilibrium of the cohesive forces would remain undisturbed: the tendency of the new angular surface of the fluid water to contract would therefore be completely destroyed by the contact of a solid of equal attractive force. If the solid were of smaller attractive force, the tendency to contract would only be proportional to the difference of the attractive forces or densities, the effect of as many of the attractive particles of the fluid being neutralised, as are equivalent to a solid of a like density or attractive power. For a similar reason, the tendency of a given fluid, to contract the sum of the surfaces of itself and a contiguous solid, will be simply as the density of the solid, or as the mutual attractive force of the solid and fluid. And it is indifferent whether we consider the pressure produced by these supposed superficial tensions, or the force acting in the direction of the surfaces to be compared. We may therefore inquire into the conditions of equilibrium of the three forces acting on the angular particles, one in the direction of the surface of the fluid only, a second in that of the common surface of the solid and fluid, and the third in that of the exposed surface of the solid. Now, supposing the angle of the fluid to be obtuse, the whole superficial cohesion of the fluid being represented by the radius, the part which acts in the direction of the surface of the solid will be proportional to the cosine of the inclination; and this force, added to the force of the solid, will be equal to the force of the common surface of the solid and fluid, or to the difference of their forces; consequently, the cosine added to twice the force of the solid, will be equal to the whole force of the fluid, or to the radius; hence the force of the solid is represented by half the difference between the cosine and the radius, or by half the versed sine; or, if the force of the fluid be represented by the diameter, the whole versed sine will indicate the force of the solid. And the same result follows when the angle of the fluid is acute. Hence we may infer, that if the solid have half

the attractive force of the fluid, the surfaces will be perpendicular; and this seems in itself reasonable, since two rectangular edges of the solid are equally near to the angular particles with one of the fluid: and we may expect a fluid to rise and adhere to the surface of every solid more than half as attractive as itself; a conclusion which Clairaut has already inferred, in a different manner, from principles which he has but cursorily investigated, in his treatise on the figure of the earth.

The versed sine varies as the square of the sine of half the angle: the force must therefore be as the square of the height to which the fluid may be elevated in contact with a horizontal surface, or nearly as the square of the number of grains expressing the apparent cohesion. Thus, according to the experiments of Morveau, on the suppositions already premised, we may infer that the mutual attraction of the particles of mercury being unity, that of mercury for gold will be 1. or more, that of silver about .94, of tin .90, of lead .81, of bismuth .72, of zinc .21, of copper .10, of antimony .08, of iron .07, and of cobalt .0004. The attraction of glass for mercury will be about one sixth of the mutual attraction of the particles of mercury: but when the contact is perfect, it appears to be considerably greater.

Although the whole of this reasoning, on the attraction of solids, is to be considered rather as an approximation than as a strict demonstration, yet we are amply justified in concluding, that all the phenomena of capillary action may be accurately explained and mathematically demonstrated from the general law of the equable tension of the surface of a fluid, together with the consideration of the angle of contact appropriate to every combination of a fluid with a solid. Some anomalies, noticed by Musschenbroek and others, respecting in particular the effects of tubes of considerable lengths, have not been considered; but there is great reason to suppose, that either the want of uniformity in the bore, or some similar inaccuracy, has been the cause of these irregularities, which have by no means been sufficiently confirmed to afford an objection to any theory. The principle which has been laid down respecting

the contractile powers of the common surface of a solid and a fluid, is confirmed by an observation which I have made on the small drops of oil which form themselves on water. There is no doubt but that this cohesion is in some measure independent of the chemical affinities of the substances concerned: tallow, when solid, has a very evident attraction for the water out of which it is raised; and the same attraction must operate upon an unctuous fluid to cause it to spread on water, the fluidity of the water allowing this powerful agent to exert itself with an unresisted velocity. An oil, which has thus been spread, is afterwards collected, by some irregularity of attraction, into thin drops, which the slightest agitation again dissipates; their surface forms a very regular curve, which terminates abruptly in a surface perfectly horizontal: now it follows from the laws of hydrostatics, that the lower surface of these drops must constitute a curve, of which the extreme inclination to the horizon is to the inclination of the upper surface, as the specific gravity of the oil to the difference between its specific gravity and that of water: consequently, since the contractile forces are held in equilibrium by a force which is perfectly horizontal, their magnitude must be in the ratio that has been already assigned; and it may be assumed as consonant both to theory and to observation, that the contractile force of the common surface of two substances is proportional, other things being equal, to the difference of their densities. Hence, in order to explain the experiments of Boyle on the effects of a combination of fluids in capillary tubes, or any other experiments of a similar nature, we have only to apply the law of an equable tension, of which the magnitude is determined by the difference of the attractive powers of the fluids.

I shall reserve some further illustrations of this subject for a work which I have long been preparing for the press, and which I flatter myself will contain a clear and simple explanation of the most important parts of natural philosophy. I have only thought it right, in the present Paper, to lay before the Royal Society, in the shortest possible compass, the particulars of an original investigation, tending to explain some facts, and

establish some analogies, which have hitherto been obscure and unintelligible.

VIII.—Additional. Extracts from Laplace, with Remarks.*

In an essay read to the Institute of France in December 1805, and published in 1806, as a supplement to the Mécanique Céleste, Mr. Laplace has advanced a theory of capillary attraction, which has led him to results nearly similar to many of those which are contained in this paper. The coincidence is indeed in some respects so striking, that it is natural, upon the first impression, to inquire whether Mr. Laplace may not be supposed either to have seen this essay, or to have read an account of its contents in some periodical publication; but upon further reflection, we cannot for a moment imagine a person of so high and so deserved a reputation as Mr. Laplace, to wish to appropriate to himself any part of the labours of others. The path which he has followed is also extremely different from that which I had taken; several of the subjects which I had considered as belonging to the discussion, have not occurred to Mr. Laplace; and it is much more flattering than surprising, that, to an assembly of philosophers not extremely anxious to attend to the pursuits of their contemporaries, investigations should be communicated, by the most distinguished of their members, as new and important, which had been presented, a year before, to a similar society in this country. In order to facilitate the comparison of the methods which have been adopted, I shall insert here a translation of some parts of Mr. Laplace's essay, which will also serve as an illustration of the theory advanced in this paper; and I shall add some remarks on the points in which those methods differ most.

"I have considered," says Mr. Laplace, "in the tenth book of this work, the phenomena derived from the refractive powers of transparent bodies acting on light. This force is the result of the attraction of their particles; but the law of this attraction cannot be determined by the phenomena, because they only require that it should be insensible at all

* The whole of this article is an addition to the original memoir.—*Ed.*

sensible distances. All possible laws of attraction, which fulfil this condition, agree equally well with the different phenomena of refraction indicated by experience, the principal of which is the constant proportion of the sine of refraction to that of incidence, in the passage of a ray of light through a transparent body. It is only in this case, that this kind of attraction has been subjected to an exact analysis. I shall now submit to the consideration of mathematicians a second case, still more remarkable than the first, on account of the variety and singularity of the phenomena which depend on it, and which may be analysed with equal accuracy; this case is that of capillary action. The effects of refractive powers belong to mechanics, and in particular to the theory of projectiles; those of capillary action relate to hydrostatics, or the equilibrium of fluids, which are raised or depressed by its means, according to certain laws, which I propose to explain."

I shall here take the liberty of observing, that the arguments which I have formerly advanced in favour of the Huygenian theory of light, would perhaps have occasioned some little hesitation with respect to the action here supposed to be exerted by transparent bodies on light, if they had ever been so fortunate as to obtain Mr. Laplace's attention. Indeed an "attraction insensible at all sensible distances" would not explain the effects of what Newton calls inflection, which affects the rays passing at a very considerable distance, at least as much as the tenth or twentieth of an inch, on each side of an opaque substance, placed in a small pencil of light in a dark room.

"Clairaut is the first, and has hitherto remained the only person, that has subjected the phenomena of capillary tubes to a rigorous calculation, in his treatise on the figure of the earth. After having shown, by arguments which are equally applicable to all the theories which have been advanced, the inaccuracy and insufficiency of that of Jurin, he enters into an exact analysis of all the forces which can contribute to the elevation of a portion of water in a tube of glass. But his theory, although explained with all the elegance peculiar to the excellent work which contains it, leaves undetermined the law of the height of that elevation, which is found from experiment to be inversely proportional to the diameter of the tube. This great mathematician contents himself with observing, that there must be an infinite variety of laws of attraction, which, if substituted in his formulas, would afford this conclusion. The knowledge of these laws is, however, the most delicate and the most important part of the theory; it is absolutely necessary for con-

necting together the different phenomena of capillary action; and Clairaut would himself have been aware of this necessity, if he had wished, for example, to pass from capillary tubes to the spaces included between two parallel planes, and to deduce from calculation the equality, which is shown by experiment, between the height of ascent of a fluid in a cylindrical tube, and its height between two parallel planes, of which the distance is equal to the semidiameter of the tube; a relation which no one has yet attempted to explain. I endeavoured, long ago, to determine the laws of attraction on which these phenomena depend; some later investigations have enabled me to demonstrate that they may all be referred to the same laws which will account for the phenomena of refraction, that is, to such as limit the sensible effect of the attraction to an insensible distance; and from these laws, a complete theory of capillary action may be deduced."

It is true that Clairaut was the first that attempted to lay the foundation of a theory of capillary action, but he is by no means the only one that has made the attempt. Segner published, in the first volume of the Transactions of the Royal Society of Göttingen, for 1751, an essay, in which he has gone much further than Clairaut: it is true that he has made some mistakes in particular cases; but he begins, like Mr. Laplace, from the effects of an attraction insensible at all sensible distances; he has demonstrated that the curvature of each point of the surface of a fluid is always proportional to its distance above or below the general level, and he has inferred, from earlier experiments, the true magnitude of this curvature at a given height, both for water and for mercury, without material error. We shall however find that the principles which Clairaut, Segner, and Laplace have successively adopted, are insufficient for explaining all the phenomena; and that it is impossible to account for them without introducing the consideration of a repulsive force, which must indeed inevitably be supposed to exist, even if its presence were not inferred from the effects of capillary action. "Attempts" have certainly been made to explain the equality of the ascent of a fluid between the two planes, and in a tube of which the radius is equal to their distance: Mr. Leslie has made such an attempt, and with perfect success; but, if I am not mistaken, the same explanation had been given long before.

"Clairaut supposes, that a capillary tube may exert a sensible action on an infinitely narrow column of the fluid, situated in the axis of the tube. In this respect, I am obliged to differ from him, and to agree with Hauksbee, and with many other philosophers, in thinking, that capillary action, like refractive powers, and the forces of chemical affinities, is only sensible at imperceptible distances. Hauksbee has observed that when the internal diameters of several capillary tubes are equal, the water rises in them to the same height, whether they are very thin or very thick. The cylindrical strata of glass, which are at a sensible distance from the interior surface, do not therefore contribute to the ascent of the water, although each of them, taken separately, would cause it to rise above its natural level. It is not the interposition of the strata which they surround, that prevents their action on the water; for it is natural to suppose, that the force of capillary attraction is transmitted through the substance of all material bodies, in the same manner as that of gravitation; this action is, therefore, only prevented by the distance of the fluid from these strata; whence it follows that the attraction of glass for water is only sensible at insensible distances.

"Proceeding upon this principle, I have investigated the action of a fluid mass, terminated by a portion of a concave or convex spherical surface, upon a fluid column within it, contained in an infinitely narrow cylindrical cavity or tube, directed towards the centre of the surface. By this action I mean the pressure, which the fluid contained in the tube would exert, in consequence of the attraction of the whole mass, upon a flat basis, situated within the tube, perpendicular to its sides, and at any sensible distance from the external surface, taking this basis for unity. I have shown that this action is either smaller or greater than if the surface were plane, accordingly as it is either concave or convex. The algebraical formula, which expresses it, consists of two terms: the first, which is much larger than the second, expresses the action of the mass supposed to be terminated by a plane surface; and I conceive that this force is the cause of the suspension of mercury in the tube of a barometer, at a height two or three times greater than that which is derived from the pressure of the atmosphere, of the refractive powers of transparent bodies, of cohesion, and of chemical affinities in general. The second term expresses that part of the attraction, which is derived from the curvature of the surface, that is, the attraction of the meniscus comprehended between that surface and the plane which touches it. This action is either added to the former, or subtracted from it, accordingly as the surface is convex or concave. It is inversely proportional to the radius of the spherical surface; and it is indeed obvious, that, the smaller the radius is, the greater is the meniscus near the point of contact. This second term expresses the cause of capillary action, which

differs, in this respect, from the chemical affinities represented by the first term."

It is indeed so "obvious," that the meniscus, which constitutes the difference between a curved surface and a plane one, is inversely proportional to the radius of curvature, that the complicated calculations, which have led Mr. Laplace to this conclusion, must be considered as wholly superfluous. The attraction of the meniscus upon the evanescent column must be confined to the edge which immediately touches the column, extending only to an insensible distance on each side; and the situation of all the particles in this infinitely thin edge of the meniscus, with respect to the column, being similar, whatever the curvature may be, it is evident that their joint action must be proportional to their number, that is, to the curvature of the surface.

"From these conclusions, relating to bodies which are terminated by sensible portions of a spherical surface, I deduce this general theorem. Whenever the attractive force becomes insensible at any sensible distance, the action of a body terminated by a curved surface, on an internal column, of infinitely small diameter, and perpendicular to the surface at any point, is equal to the half sum of the actions, which would be exerted on the same column by two spheres, having for their radii the largest and the smallest of the radii of curvature at the given point."

This theorem may be very simply inferred from the former, by considering that, according to the principle laid down in the second section of this essay, the sum of the thicknesses of the evanescent meniscoid, in any two planes passing through the axis at right angles to each other, is equal to the sum of the thicknesses of the two menisci formed by the largest and the smallest radii of curvature; consequently the sum of the whole actions of these menisci must be twice as great as the action of the meniscoid.

"By means of this theorem, and of the laws of the equilibrium of fluids, we may determine the figure which must be assumed by a gravitating fluid, inclosed in a vessel of any given form. We obtain from these principles an equation of partial differences of the second order, the

integral of which cannot be found by any known method. If the figure is such, as might be formed by the revolution of a curve round an axis, the equation is reduced to common differences or fluxions, and its integral or fluent may be found very near the truth, when the surface is very small. I have shown in this manner, that, in very narrow tubes, the surface of the fluid approaches the nearer to that of a sphere, as the diameter of the tube is smaller. If these segments are similar, in different tubes of the same substance, the radii of their surfaces will be directly proportional to the diameters of the tubes. Now this similarity of the spherical segments will easily appear, if we consider that the distance, at which the action of the tube ceases to be sensible, is imperceptible; so that if, by means of a very powerful microscope, it were possible to make it appear equal to the thousandth part of a metre, it is probable, that the same magnifying power would augment the apparent diameter of the tube to several metres. The surface of the tube may therefore be considered as nearly plane, within the limits of a circle equal in radius to the distance at which its attraction becomes sensible; consequently the fluid within this distance will be elevated or depressed with respect to the surface of the tube, almost precisely in the same manner as if it were perfectly plane. Beyond this distance, the fluid being subjected to no other sensible action than that of gravitation, and that of its own attraction, the surface will be very nearly that of a spherical segment, the marginal parts of which corresponding with those of the surface of the fluid at the point which is the limit of the sphere of the sensible activity of the tube, will be inclined very nearly in the same angle to its surface, whatever its magnitude may be: hence it follows, that all these segments will be similar."

The "near approach" of the surface of a fluid in a very small tube to a portion of a sphere is sufficiently obvious from the fundamental principle, that the curvature is proportional to the height above the general surface of the fluid; for if the diameter of the tube be small, this height will be so considerable, that its variation at any part of the concave or convex surface may be disregarded, and the curvature may consequently be considered as uniform throughout the surface. It is only upon the supposition of a surface nearly approaching to a spherical form, that Mr. Laplace has endeavoured to determine the "integral, very near the truth." He has deduced from the expression, which indicates the curvature of the surface, another which is simpler, and which might easily have been inferred at once from the uniform tension of the surface, as supporting at

each point the weight of the portion of the fluid below it: he has then supposed this weight to be the same as if the surface were spherical, and has deduced from this supposition an approximate expression, for the elevation corresponding to a given angular position of the surface only. This formula is however still only applicable to those cases, in which the surface may be considered as nearly spherical; and in these it is superfluous. For example, if the surface of the mercury in a barometer be depressed one twentieth of an inch, as it actually is in a tube somewhat less than a quarter of an inch in diameter, Mr. Laplace's formula fails so completely, as to indicate a concavity instead of a convexity; for α being the reciprocal of what I have called the appropriate rectangle, and θ being 50°, the term αb^2 becomes $= 4$, and makes the negative part of the formula greater than the positive. When Mr. Laplace investigates the relation of the curvature and of the marginal depression to the diameter of the tube, he simply considers the whole surface as spherical; but even on this supposition his formula is by no means the most accurate that may be found, and begins to be materially incorrect even when the diameter of the tube amounts to one-fifth of an inch only. The formula which I have already given in this paper is sufficiently accurate, until the diameter becomes equal to half an inch; but I shall hereafter mention another, which comes much nearer to the truth in all cases.

"The comparison of these results shows the true cause of the ascent or depression of fluids in capillary tubes, which is inversely proportional to their diameters. If we imagine an infinitely narrow inverted siphon to have one of its branches placed in the axis of the tube of glass, and the other terminating in the general horizontal surface of the water in the vessel, the action of the water in the tube on the first branch of the siphon will be less, on account of the concavity of its surface, than the action of the water of the vessel on the second; the fluid must therefore ascend in the tube, in order to compensate for this difference; and, as it has been shown, that the difference of the two actions is inversely proportional to the diameter of the tube, the elevation of the fluid above the general level must follow the same law.

"If the surface of the fluid within the tube is convex, as in the case of mercury contained in a tube of glass, its action on the inverted siphon

will be greater than that of the fluid in the vessel; the fluid must therefore be depressed in the tube, in proportion to the difference, that is, inversely in proportion to the diameter of the tube.

"It appears, therefore, that the immediate attraction of a capillary tube has no other effect on the elevation or depression of the fluid contained in it, than so far as it determines the inclination of the first portion of the surface of the fluid, when it approaches the sides of the tube: and that the concavity or convexity of the surface, as well as the magnitude of its curvature, depends on this inclination. The friction of the fluid, against the sides of the tube, may increase or diminish a little the curvature of its surface, as we continually observe in the mercury of the barometer; and in this case the capillary effects are increased or diminished in the same proportion. These effects are also very sensibly modified by the co-operation of the forces derived from the concavity and convexity of two different surfaces. It will appear hereafter, that water may be raised, in a given capillary tube, to a greater height above its natural level in this manner, than when the tube is immersed in a vessel filled with that fluid."

It would perhaps be more correct to say in this case "above its apparent level:" for the real horizontal surface must here be considered as situated above the lower orifice of the tube, the weight of the portion of the fluid below it being as much supported by the convexity of the surface of the drop, as if it were contained in a vessel of any other kind.

"The fluxional equation of the surface of a fluid, inclosed in a capillary space of any kind, which may be referred to an axis of revolution, leads to this general result, that if a cylinder be placed within a tube, so that its axis may coincide with that of the tube, the fluid will rise in this space to the same height, as in a tube of which the radius is equal to this distance. If we suppose the radii of the tube and of the cylinder to become infinite, we obtain the case of a fluid contained between two parallel vertical planes, placed near each other. The conclusion is confirmed in this case by the experiments which were made long ago in the presence of the Royal Society of London, under the inspection of Newton, who has quoted them in his Optics; that admirable work, in which this profound genius, looking forwards beyond the state of science in his own times, has suggested a variety of original ideas, which the modern improvements of chemistry have confirmed. Mr. Haüy has been so good as to make, at my request, some experiments on the case which constitutes the opposite extreme, that is, with tubes and cylinders of a very small diameter, and he has found the conclusion as correct in this case, as in the former."

If indeed we may be allowed to place any confidence in the fundamental principle of an equable tension of the surface of the fluid, an equal length of the line of contact of the solid and fluid supporting in all cases an equal weight, these results follow of necessity, without any intricacies of calculation whatever.

"The phenomena exhibited by a drop of a fluid, moving, or suspended in equilibrium, either in a conical capillary tube, or between two planes, inclined at a small angle to each other, are extremely proper to confirm our theory. A small column of water, in a conical tube, open at both ends, and held in a horizontal position, will move towards the vertex of the cone; and it is obvious that this must necessarily happen. In fact, the surface of the column is concave at both ends, but the radius of this curvature is smaller at the end nearer the vertex than at the opposite end; the action of the fluid upon itself is therefore less at the narrower end, consequently the column must be drawn towards this side. If the fluid employed be mercury, its surface will be convex, and the radius of curvature will still be smaller towards the vertex than towards the base of the cone; but, on account of its convexity, the action of the fluid upon itself will be greater at the narrower end, and the column must therefore move towards the wider part of the tube.

"This action may be counterbalanced by the weight of the column, so as to be held in equilibrium by it, if we incline the axis of the tube to the horizon. A very simple calculation is sufficient to demonstrate that if the length of the column is inconsiderable, the sine of the inclination of the axis must be inversely proportional to the square of the distance of the middle of the column from the summit of the cone; and this law is equally applicable to the case of a drop of a fluid placed between two planes, which form a very small angle with each other, their horizontal margins being in contact. These results are perfectly conformable to experiment, as may be seen in the 31st query of Newton's Optics. This great geometrician has endeavoured to explain them, but his explanation, compared with that which has been here advanced, serves only to show the advantages of a precise and mathematical investigation."

Mr. Laplace's superior skill in the most refined "mathematical investigations" might perhaps have enabled him to make still more essential improvements, if it had been employed on some other subjects of natural philosophy; but his explanation of these phenomena being exactly the same as that which I had already published, in an essay not containing, in its original

state, any one mathematical symbol, it is obvious that the inaccuracy of Newton's reasoning did not depend upon any deficiency in his mathematical acquirements.

"It may be shown by calculation, that the sine of the inclination of the axis of the cone to the horizon will be very nearly equal to the fraction of which the denominator is the distance of the middle of the drop from the summit of the cone, and the numerator the height to which the fluid would rise in a cylindrical tube, of a diameter equal to that of the cone at the middle of the column. If the two planes, inclosing a drop of the same fluid, form with each other an angle, equal to that which is formed by the axis of the cone and its sides, the inclination of a plane, bisecting this angle, to the horizon, must be the same as that of the axis of the cone, in order that the drop may remain in equilibrium. Hauksbee has made, with very great care, an experiment of this kind, which I have compared with the theorem here laid down; and the near agreement between the experiment and the theorem is amply sufficient to confirm its truth."

If the height at which the fluid would stand, in a tube of the diameter of the upper end of the column, be h; the distance of this end from the vertex of the cone being x, and the length of the column y, the height corresponding to the remoter end will be $\frac{hx}{x+y}$, and the difference of the heights $h - \frac{hx}{x+y} = \frac{hy}{x+y}$, which must be the difference of the heights of the ends of the drop, in order that it may remain in equilibrium; but this height is to y as h to $x + y$, consequently the axis of the tube must be inclined to the horizon in an angle, of which the sine is exactly $\frac{h}{x+y}$; the denominator being the distance of one end of the vertex, and the numerator the height at which the fluid would stand in a tube, of which the diameter is equal to that of the column at the other end.

"This theory affords us also an explanation of another remarkable phenomenon, which occurs in experiments of this nature. If a fluid be either elevated or depressed between two vertical and parallel planes, of which the lower ends are immersed in the fluid, the planes will tend to approach each other. It is shown, by calculation, that if the fluid is elevated between them, each plane is subjected to a pressure, urging it towards the other plane, equal to that of a column of the same fluid, of a height equal to the half sum of the elevations of the internal and ex-

ternal lines of contact, of the surface of the fluid with the plane, above the general level, and standing on a base equal to a part of the plane included between these lines. If the fluid is depressed between the planes, each of them will be forced inwards, by a pressure equal to that of a column of the same fluid, of which the height is half the sum of the depressions of the lines of contact of the external and internal surfaces of the fluid with the plane, and its base the part of the plane comprehended between those lines."

In another part of his essay, Mr. Laplace asserts that "this force increases in the inverse ratio of the distance of the planes." If this is not an error of the press, or of the pen, it can only mean that the force increases as the distance diminishes; for the magnitude of the force is not simply in the inverse ratio of the distances, but very nearly in the inverse ratio of their squares, as I have already observed.

"Since it has been hitherto usual with natural philosophers, to consider the concavity and convexity of the surfaces of fluids in capillary spaces, as a secondary effect of capillary attraction only, and not as the principal cause of phenomena of this kind, they have not attached much importance to the determination of the curvature of these surfaces. But the theory, which has been here advanced, having shown that all these phenomena depend principally on the curvature, it becomes of consequence to examine it. Several experiments, which have been made with great accuracy by Mr. Haüy, have shown, that in capillary tubes of glass, of very small diameters, the concave surfaces of water and of oils, and the convex surfaces of mercury, differ very little from the form of a hemisphere."

Mr. Laplace informs us that MM. Haüy and Trémery made at his request several experiments, in which the mean ascent of water, in a tube one thousandth part of a metre in diameter, was 13.57 thousandths, and that of oil of oranges 6.74. The product of the diameter and the height of ascent of water is $.039371 \times .534 = .021$ E. i., which is little more than half as much as I have assigned for this product from the best experiments of many other observers. Probably both these experiments, and those of Newton or Hauksbee, were made with tubes and plates either a little greasy or too dry; and Mr. Haüy might be the more readily satisfied with the first results that he obtained, from finding them agree nearly with those of

Newton, which Mr. Laplace wished to compare with them. These gentlemen also found the depression of mercury in a tube of the same diameter .2887 E. i., the product being .01137, instead of .015, which is the ultimate product inferred from Lord Charles Cavendish's experiments of a similar nature. The observation of Mr. Haüy, on the curvature of the surface of mercury in a tube, is also far from being accurate; Mr. Laplace himself asserts that the angular extent of the surface must fall short of that of a hemisphere more or less, accordingly as the tube has more or less attraction for the fluid; and it is easy to show that glass has a very considerable attraction for mercury. The method that I took to ascertain the angle, formed by the surface of the mercury, with the side of the tube, was to observe in what position the light reflected from it began to reach the eye; and I have every reason to think, from the comparison of a great variety of experiments of different kinds, that the angle which I have assigned is very near the truth.

I have lately repeated my calculations of the depression of mercury, in barometer tubes of considerable diameter, with great care, and by different methods. I had before formed a table, by means of diagrams, which I had actually constructed, for each case, upon a sufficiently accurate approximation: I have now followed nearly the same steps in calculating, by means of tables of sines and cosines, the precise form of the surface in a variety of cases. Beginning from the vertex of the curve, I have determined the mean curvature for every small arc, from the approximate height of its middle point; calculating with the assistance of a series of differences, the normal of the curve at each step for the same point, in order to find the transverse curvature. I have also pursued, in some cases, in order to confirm these calculations, a method totally different, finding the mass of the quantity of fluid to be supported by the tension of the surface at each concentric circle, and inferring from its magnitude the inclination of the curve to the horizon; taking the height of the external circumference of each portion, thus calculated, for the mean height; a supposition which nearly compensates for the omission of the curvature of its sur-

face. But the accumulated effect of this curvature becomes very sensible in the vertical height of the surface, and I have therefore allowed for it, upon the supposition of a simple curvature varying with the height; but this correction, for want of including the effect of the variation of the transverse curvature, is still a little too small; the horizontal diameter of the surface, however, agrees extremely well with the former mode of calculation. In order that the results of these investigations may be the more easily compared with each other and with experiment, I shall insert some specimens, by means of which, if it be required, the curves may be very correctly delineated.

1. Central Depression .007.

FIRST METHOD, BY THE CURVATURE.			SECOND METHOD, BY THE TENSION.		
Arc.	Horizontal ordinate.	Depression.	Arc.	Horizontal ordinate.	Depression.
0°	.00000	.00700	.00°	.00000	.00700
1	.02444	.00721	.02	.02000	.00714
2	.04758	.00782	.04	.04000	.00757
3	.06651	.00865	.06	.05999	.00830
4	.08338	.00968	.08	.07997	.00939
5	.09791	.01082	.10	.09993	.01101
6	.11049	.01203	.12	.11985	.01302
7	.12153	.01329	.14	.13971	.01566
8	.13146	.01458	.16	.15948	.01909
9	.14022	.01589	.18	.17908	.02353
10	.14814	.01721	.20	.19842	.02922
12	.16177	.01986	.22	.21732	.03653
14	.17338	.02254	.24	.23550	.04530
16	.18344	.02524	.26	.25039	.05707
18	.19229	.02793	.2705	.25740	.06460
20	.20012	.03063			
25	.21603	.03722			
30	.22869	.04381			
35	.23895	.05033			
40	.24731	.05676			
45	.25420	.06307			
50	.25986	.06911			

2. Central Depression .05.

FIRST METHOD.			SECOND METHOD.		
Arc.	Horizontal ordinate.	Depression.	Arc.	Horizontal ordinate.	Depression.
0°	.00000	.05000	.00°	.00000	.05000
1	.00349	.05003	.01	.01000	.05025
2	.00697	.05012	.02	.01999	.05101
3	.01044	.05027	.03	.02994	.05229
4	.01388	.05048	.04	.03982	.05409
5	.01729	.05075	.05	.04961	.05644
6	.02068	.05107	.06	.05926	.05938

FIRST METHOD—*continued.*

Arc.	Horizontal ordinate.	Depression.
7°	.02402	.05145
8	.02731	.05189
9	.03056	.05237
10	.03375	.05291
12	.03995	.05411
14	.04589	.05543
16	.05157	.05696
18	.05697	.05861
20	.06209	.06037
25	.07363	.06515
30	.08365	.07036
35	.09224	.07583
40	.09958	.08146
45	.10581	.08717
50	.11105	.09289

SECOND METHOD—*continued.*

Arc.	Horizontal ordinate.	Depression.
.07°	.06873	.06294
.08	.07796	.06718
.09	.08688	.07212
.10	.09540	.07783
.11	.10342	.08436
.12	.11080	.09170
.1214	.11173	.09280

3. Central Depression .14.

FIRST METHOD.

Arc.	Horizontal ordinate.	Depression.
0°	.00000	.14000
5	.00623	.14027
10	.01234	.14108
15	.01832	.14240
20	.02405	.14421
25	.02950	.14646
30	.03459	.14911
35	.03931	.15211
40	.04361	.15541
45	.04749	.15897
50	.05091	.16270

SECOND METHOD.

Arc.	Horizontal ordinate.	Depression.
.00°	.00000	.1400
.01	.01000	.1407
.02	.01990	.1428
.03	.02950	.1464
.04	.03857	.1514
.05	.04686	.1580
.0555	.05078	.1621

For representing the depression, thus determined, in a formula capable of expressing it at once, in terms of the diameter of the tube, I have deduced an approximate determination from the supposition of a spherical surface, and corrected it, by comparison with the results of these calculations, so as to agree with them all, without an error of one two-thousandth of an inch, in the most unfavourable of the five cases compared. The theorem is, first, $e = \frac{.015d}{dd + .16}$, which is nearly half the versed sine of a spherical surface, and then $f = \frac{.015}{d} - \frac{3}{4}e - 14.5e^3$, which shows the central depression without any sensible error.

I have also found a formula, which expresses the difference between the central and marginal depression, so that an observation on the height of the barometer may be corrected, with equal accuracy, whether the elevation of the highest

or lowest point of the surface has been measured, provided that the tube be of moderate dimensions. This formula is $g = \frac{5d + 100d^8}{15\ (5d + 100d^3) + 18}$. If d were very large, it would require some further correction, g being ultimately too great by .0069. The results of these formulas are compared, in the first of the following tables, with those of the calculations at large; and in the second, they are reduced into a form more immediately applicable to practice, and are compared also with the table published by Mr. Cavendish.

Diameter.	True central depression.	Form 1.	True additional depression at the margin.	Form 2.
.5197	.007	.0071	.0621	.0622
.3187	.025	.0250	.0535	.0534
.2221	.050	.0498	.0429	.0432
.1468	.090	.0905	.0313	.0311
.1018	.140	.1396	.0227	.0226

Diameter.	Observed central depression.	True central depression.	True marginal depression.
1.00		.0022	
.90		.0023	
.80		.0026	
.70		.0032	
.60	.005	.0045	.0680
.50	.007	.0074	.0691
.45		.0100	.0703
.40	.015	.0139	.0722
.35	.025	.0196	.0753
.30	.036	.0280	.0798
.25	.050	.0404	.0872
.20	.067	.0589	.0989
.15	.092	.0880	.1196
.10	.140	.1422	.1646
.05		.2964	.3083

By continuing the calculations of the figure of some of these curves to an arc of 90°, I have adapted them to the surface of water contained in a cylindrical tube; but in this case the scale must be supposed to be augmented in the proportion of 1 to $\sqrt{2}$. The additional numbers stand thus in abstract.

1. Central Depression .025.

Arc.	Horizontal ordinate.	Depression.	Arc.	Horizontal ordinate.	Depression.
0°	.00000	.02500	50°	.15934	.07847
10	.06214	.03023	60	.16768	.09039
20	.10280	.04097	70	.17296	.10169
30	.12969	.05340	80	.17580	.11228
40	.14793	.06606	90	.17665	.12203

2. Central Depression .05.

Arc.	Horizontal ordinate.	Depression.	Arc.	Horizontal ordinate.	Depression.
0°	.00000	.05000	50°	.11105	.09289
10	.03375	.05291	60	.11911	.10414
20	.06209	.06037	70	.12494	.11492
30	.08365	.07036	80	.12769	.12518
40	.09958	.08146	90	.12853	.13470

3. Central Depression .09.

Arc.	Horizontal ordinate.	Depression.	Arc.	Horizontal ordinate.	Depression.
0°	.00000	.09000	50°	.07340	.12133
10	.01904	.09042	60	.08022	.13106
20	.03662	.09366	70	.08475	.14077
30	.05172	.10337	80	.08727	.15017
40	.06397	.11192	90	.08804	.15904

Hence, for water, we have the central elevation .035355, .07071, and .12728, and the marginal elevation .17258, .19050, and .22495, in tubes of which the diameters are .49964, .36354, and .2490 respectively. The difference of the elevations is expressed nearly by $h = \frac{\sqrt{2}\,(d + 100d^4)}{\sqrt{8 + 10\,(d + 100d^4)}}$, which is correct in the extreme cases on both sides, and becomes, when d is .25, and .5, .098, and .136 respectively, instead of .0977 and .137; and when $d = 1$, $h = .141$.

"Clairaut," says Mr. Laplace, "has made this singular remark; that if the law of the attraction of the matter of the tube, for the fluid, differs only in its intensity from that of the attraction of the particles of the fluid among themselves, the fluid will be elevated above the level, as long as the intensity of the first of these forces exceeds half that of the second. If it be exactly half as great, it may easily be shown, that the surface of the fluid in the tube will be horizontal, and that it will not be raised above the level. If the two forces be equal, the surface of the fluid will be concave and hemispherical, and it will be elevated within the tube. If the intensity of the attraction of the tube be wholly wanting or insensible, the surface of the fluid will be hemispherical, but it will be convex and depressed. Between these two limits, the surface will be that of a segment of a sphere, and it will be either concave or convex, accordingly as the intensity of the attraction of the matter of the tube for the fluid is greater or less, than half of that of the mutual attraction of the particles of the fluid."

These conclusions are in all probability nearly correct with respect to very small tubes; but it is remarkable that they are not fairly deducible from Mr. Laplace's principles, nor

from those of Clairaut, whose steps he has followed; and that the expression, which he has derived from them, as indicating the condition of equilibrium of the surface of a fluid inclined to that of a solid, implies, by including an impossibility, that such an equilibrium cannot subsist. This equation requires that the attraction of the fluid, contained between the surface and its extreme tangent, be more than equal to the difference of the attraction of the two rectangular portions composing the flat solid, and one similar portion of the fluid, reduced only in the ratio of the sine of the angle occupied by the termination of the fluid, to the radius: but it is very evident that the action of the portion of the fluid, thus cut off by the tangent, must be utterly evanescent, in comparison with the other forces concerned, especially if we consider that the surface of the fluid, as well as of that of the tube, within the distance "of the sphere of activity of the attraction" is, to use Mr. Laplace's terms, "almost absolutely plane." There can therefore be no equilibrium upon these principles, when the density of the solid is greater or less than half that of the fluid, unless the surface of the fluid have a common tangent with that of the solid: while, on the other hand, when the densities are in this proportion, the surface will remain in equilibrium in any position; the action of the fluid being always proportional to the chord of its angular extent, and composing, when combined with that of the solid, a result perpendicular to the surface. If Mr. Laplace had attempted to confirm or to confute my reasoning, respecting the mutual attractions of solids and fluids, he would probably have discovered the insufficiency of these principles, and would perhaps have been induced to admit my explanation of the foundation of the laws of superficial cohesion, as derived from the combination of an attractive with a repulsive force, varying according to a different law.

"If the intensity of the attraction of the tube for the fluid exceeds that of the attraction of the fluid for its own particles, I think it probable that, in this case, the fluid attaching itself firmly to the tube, forms of itself an interior tube, which alone raises the fluid, so as to make its surface a concave hemisphere. It may reasonably be conjectured, that this is the case with water and with oils, in tubes of glass.

"The elevation of fluids between two vertical planes, which form

very small angles with each other, and their discharge through capillary siphons, present a variety of phenomena, which are so many corollaries from my theory. On the whole, if any person will take the trouble of comparing it with the numerous experiments which have been made on capillary action, he will see that the results of these experiments, when made with proper precaution, may be deduced from it, not by vague considerations, which always leave the subject in uncertainty, but by a series of geometrical arguments, which appear to me to remove every doubt respecting the truth of the theory. I wish that this application of analytical reasoning, to one of the most curious departments of natural philosophy, may be thought interesting by mathematicians, and may induce them to make further attempts of a similar nature. Besides the advantage of adding certainty to physical sciences, such investigations tend also to the improvement of the mathematics themselves, since they frequently require the invention of new methods of calculation."

It must be confessed that, in this country, the cultivation of the higher branches of the mathematics, and the invention of new methods of calculation, cannot be too much recommended to the generality of those who apply themselves to natural philosophy; but it is equally true, on the other hand, that the first mathematicians on the continent have exerted great ingenuity in involving the plainest truths of mechanics in the intricacies of algebraical formulas, and in some instances have even lost sight of the real state of an investigation, by attending only to the symbols, which they have employed for expressing its steps.*

* Laplace published, in 1807, a second 'Supplément à la Théorie de l'Action capillaire,' in which he notices in the following terms the researches of Dr. Young:— "Lorsque je m'occupais de cet objet, M. Thomas Young en faisait pareillement le sujet de recherches ingénieuses qu'il a insérées dans les 'Transactions Philosophiques' pour l'année 1805. En comparant, avec Segner, la force capillaire, à la tension d'une surface, qui envelopperait les liquides, et en appliquant à cette force les résultats connus sur la tension des surfaces, il a reconnu qu'il fallait avoir égard à la courbure des surfaces liquides, dans deux directions perpendiculaires entre elles; il a de plus supposé que ces surfaces, pour un même liquide, coupent sous le même angle les parois des tubes formés de la même matière, quelle que soit d'ailleurs leur figure; ce qui, comme on l'a vu, cesse d'être exact aux extrémités de ces parois. Mais il n'a pas tenté, comme Segner, de dériver ces hypothèses de la loi de l'attraction des molecules, décroissante avec une extrême rapidité; ce qui était indispensable pour les réaliser. Elles ne pouvaient l'être que par une démonstration rigoureuse, pareille à celles que nous avons données dans la première méthode à laquelle les explications de Segner et de M. Thomas Young se rattachent, comme celle de Jurin se rattache à la seconde méthode."

These Supplements were made the subject of a Review by Dr. Young, in the first number of the 'Quarterly Review,' for February, 1809. As this critique, however, contains no observations of importance beyond those which are embodied in the preceding additions to this Essay, it has not been thought necessary to reprint it.—*Note by the Editor.*

No. XX.

COHESION.*

From the Supplement to the Encyclopædia Britannica.

THE corpuscular forces, on which the mechanical properties of the aggregates of matter depend, have been in some measure considered, as far as they relate to solids, in the articles BRIDGE and CARPENTRY of this *Supplement* (Nos. LII. and LIII. in Vol. II. of this work): there are, however, other modifications of these forces, which are principally exemplified in the COHESION OF FLUIDS (No. XIX.); and which afford us a series of phenomena, highly interesting to the mathematician, on account of the difficulty of investigating their laws, and of considerable importance to the natural philosopher, from the variety of forms in which they present themselves to his observation.

SECTION I.—*Fundamental Properties of the Cohesion of a Single Fluid.*

The three states of elastic fluidity, liquidity, and solidity, in all of which the greater number of simple bodies are capable of being exhibited at different temperatures, are not uncommonly conceived to depend on the different actions of heat only, giving a repulsive force to the particles of gases, and simply detaching those of liquids, from that cohesion with the neighbouring particles, which is supposed to constitute solidity. But these ideas, however universal, may be easily shown to be totally erroneous: and it will readily be found, that the immediate

* There appears in the same Supplement, under the title of 'Elevation of Fluids,' an article on the same subject by Mr. Ivory. It reproduces, under a more simple form, the investigations of Laplace, and almost entirely ignores the previous as well as the nearly contemporary researches of Dr. Young. "*If the truth,*" says he, "*is to be told,* it may be affirmed, that reckoning back from the present time to the speculations of the Florentine Academicians, the formula of Laplace and the remark of Professor Leslie, relating to the lateral force, are the only *approaches* which have been made to a sound *physical* account of the phenomena." The article in the text was written in the summer of 1816.—*Note by the Editor.*

effect of heat alone is by no means adequate to the explanation of either of the changes of form in question.

There can never be rest without an equilibrium of force: and if two particles of matter attract each other, and yet remain without motion, it must be because there exists also a repulsive force, equal, at the given distance, to the attractive force. If we imagine the atoms of matter to be impenetrable spheres, only resisting when their surfaces come into actual contact, it would follow, that the degree of repulsive force exerted at the same distance must be capable of infinite variation, so as to counterbalance every possible modification of the attractive force, that could operate between the particles. In this there would be no mathematical absurdity, and it may sometimes even be convenient to admit the hypothesis as an approximation: but we know from physical considerations that the actual fact is otherwise. The particles of matter are by no means incompressible: the repulsion varies indeed very rapidly when they approach near to each other; but the distance of the particles, and the density of the substance, must inevitably vary, in some finite degree, from the effect of every force that tends to produce either compression or expansion.

In elastic fluids, the law of the repulsive force of the particles is perfectly ascertained; and it has been shown to vary very accurately in the inverse ratio of their mutual distances. It is natural to inquire whether this repulsive force, continued according to the same law, would be capable of affording the resistance exhibited by the same bodies in a liquid or solid form, and holding the cohesive force in equilibrium: but in order to answer this question, it would be necessary to determine the law of the variation of the cohesive force with the variation of the density. Now if this force extended to all particles within a given distance of each other, whatever the density might be, the number of particles similarly situated within the sphere of action being as the density, and each one of this number being attracted by an equal number, the whole cohesion urging any two particles to approach each other would obviously, as Laplace has observed, be as the square of the density: but since this cohesive force would increase, with the increase of

density accompanying compression, more rapidly than any repulsive force like that of elastic fluids, there could never be an equilibrium between forces thus constituted: for, as Newton has justly remarked, the force of repulsion must be supposed to affect the particles immediately contiguous to each other only, their number not increasing with the density. Nor is there any reason to infer, from the phenomena of cohesion, that this force extends to a given minute distance, rather than to a given number of particles, as that of repulsion appears to do. It would indeed be possible to assign a law for the variation of cohesion, which would reduce the repulsion of liquids and of elastic fluids to the action of the same force, without any other modification than that which depends on the mutual distance of the particles; but this law is in itself so improbable, that it cannot be considered as affording an admissible explanation of the phenomena; for it would be required that the force of cohesion should diminish, instead of increasing, with every increase of density, and with a rapidity nineteen times as great as the repulsion increased. For the height of the modulus of elasticity of all kinds of gaseous substances remaining unaltered by pressure, that of steam would still be only one twentieth as high as the modulus of elasticity of water, even if the steam were compressed by 1200 atmospheres; and the resistance to any minute change of dimensions would be twenty times as great in water as in steam of equal density, and the variation of the repulsion would be in the same proportion. It is therefore simplest to suppose the repulsion itself to be also twenty times as great, and the cohesion little or not at all altered by the effect of a slight compression or extension: and we shall have no difficulty in imagining this abrupt change in the magnitude of the repulsive force to depend on an increase of the number of particles to which it extends; supposing that when cohesion begins to affect them, this number becomes four or five times as great as before, and that it is not further increased by a greater increase of density; although, like the distance to which the force of cohesion itself extends, it may be liable to some modification from the effects of a change of temperature. Thus it is probable that the number of particles co-operating, both in

repulsion and in cohesion, is diminished by the effect of heat; for the diminution of the elasticity of a spring is much more than proportional to the expansion of its substance, although the primitive repulsive force of the single particles may very possibly be as much augmented by an elevation of temperature in this case as in that of an elastic fluid: the cohesive powers of liquids are also diminished by heat, and indeed in a considerably greater degree than the stiffness of springs, although there can be no doubt that there is a considerable analogy in these changes. However this may be, it appears that the force of cohesion cannot be supposed to vary much with the density, and it is therefore allowable to consider it as constant, at all distances, as far as its action extends; while that of repulsion, though it may operate in some degree at distances somewhat greater, may still be considered, on account of its greater intensity at smaller distances, as equivalent to a resistance terminating at a more minute interval than that to which the action of cohesion extends.

The distance at which cohesion commences between the particles of gaseous fluids appears to depend entirely on the temperature, and for any one fluid it is generally reduced to one half by an elevation of about 100° of Fahrenheit. In whatever way the particles are caused to approach nearer than this distance to each other, they become subject to the action of this force, and rush together with violence, and with a great extrication of heat, until the increased repulsion affords a sufficient resistance to the cohesion, and the gas is converted into a liquid. Superficial observers have sometimes imagined, that liquids possessed little or no cohesion; and it has generally been supposed that their cohesive powers are far inferior to those of solids. But that all liquids are more or less cohesive, is sufficiently shown by their remaining attached, in small portions, to every substance capable of coming into intimate contact with them, in opposition to the effect of gravitation, or of any other force: and the cohesion of mercury is still more fully exemplified by the well known experiment of a column, standing at a height much exceeding that of the barometer, when it has been brought, by strong agitation or otherwise, into

perfect contact with the summit of the tube, and is then raised into a vertical position; the summit of the tube supporting, or rather suspending the upper parts, and each stratum the stratum immediately below it, with a force determined by the excess of its height above that of the column equivalent to the atmospherical pressure. The perfect equality of the cohesion of a given substance in the states of solidity and liquidity, appears, however, only to have been asserted in very modern times; and the assertion has only been confirmed by a single observation of the sound produced by a piece of ice, compared with the elasticity exhibited in Canton's experiments on the compressibility of water; the results demonstrating that the resistance is either accurately or very nearly equal in both cases.

The real criterion of solidity is the lateral adhesion, which prevents that change of internal arrangement, by which a fluid can alter its external dimensions without any sensible difference in the mutual distances of its particles taken collectively, and consequently without any sensible resistance from the force of cohesion. It is probable that this lateral adhesion depends upon some symmetrical arrangement of the constituent parts of the substance, while fluidity requires a total independence of these particles, and an irregularity of situation affording a facility of sliding over each other with little or no friction. The symmetry of arrangement, when continued uniformly to a sensible extent, is readily discoverable by the appearance of crystallization; but there are several reasons for supposing it to exist, though with perpetual interruptions, in more uniform masses, or in amorphous solids. It is obvious that the lateral adhesion, confining the particles so as to prevent their sliding away, performs an office like that of the tube of a barometer to which the mercury adheres, or like that of the vessels employed by Canton and Zimmerman for confining water which is compressed; and enables the cohesive and repulsive powers of the substances to be exhibited in their full extent. Nor can we obtain any direct estimate of these powers from the slight cohesion exhibited, in some circumstances, by liquids in contact with the surface of a solid which is gradually raised, and carries with it a certain portion of the liquid; an experiment

which had been often made, with a view of determining the mutual attractions of solids and fluids, but which was first correctly explained, as Laplace observes, by our countryman Dr. Thomas Young, from its analogy with the phenomena of capillary tubes.

There are, however, still some difficulties in deducing these phenomena from the elementary actions of the forces concerned, whatever suppositions we may make respecting their primitive nature. The intermediate general principle of a hydrostatic force or pressure, proportional to the curvature of the surface, had been employed long ago by Segner, and had been considered by him as the result of corpuscular powers extending to an insensible distance only. But Segner's reasoning on this point is by no means conclusive, and he has very unaccountably committed a great error, in neglecting the consideration of the effects of a double curvature. There is also an oversight in some of the steps of the demonstration attempted by Dr. Young in his *Lectures*, which has been pointed out by an anonymous writer in Nicholson's *Journal:* and Mr. Laplace's final equation for determining the angle of contact of a solid and a liquid, which Dr. Young had first shown to be constant, has been considered as completely inaccurate, and as involving an impossibility so manifest, as to destroy all confidence in the theory from which it was deduced. A demonstration, which appears to be less exceptionable, was lately published in the *Philosophical Magazine;* and it may serve with some further illustrations for the present purpose.

It is only necessary to consider the actions of such of the particles of the liquid, as are situated at a distance from the surface shorter than that, to which the cohesive force extends; for all those which are more internal, must be urged equally in all directions by the actions of the surrounding particles. Now it will readily be perceived, that the first or outermost stratum of particles will cohere very weakly with the stratum below it, having only its own attraction to bind it down; that the second will be urged by a force nearly twice as great; and that the cohesion will gradually augment, by increments continually diminishing, until we arrive at the depth of the whole interval

to which the force extends: and below this it will remain constant, the number of particles within the given distance not undergoing any further change. It has been observed by Mr. Laplace, that this partial diminution of the density of the surface is likely to be concerned in facilitating the process of evaporation; and it has been cursorily suggested in another quarter that the polarisation of light by oblique reflection may be in some measure influenced by this gradation of density. But its more immediate effect must be to produce that uniform tension of the surface, which constitutes so important a principle in the phenomena of capillary action; for since the cohesion in the direction of the surface is the undiminished result of the attractions of the whole number of particles constituting the stratum, acting as they would do in any other part of the substance, it follows that a small cubical portion of the liquid, situated in any part of the space which we are considering, will be pressed laterally by the whole force of cohesion, but above and below by that part only, which is derived from the action of the strata above it; so that this minute portion must necessarily tend to extend itself upwards and downwards and to thicken the superficial film; and at the same time to become thinner in the direction of the surface, and to shorten it in all its dimensions; unless this alteration be prevented by some equivalent tension acting in a contrary direction: and this tension must be always the same in the same liquid, whatever its form may be, the thickness of the whole stratum being always extremely minute in comparison with any sensible radius of curvature.

Upon these grounds we may proceed to determine the actual magnitude of the contractile force derived from a given cohesion extending to a given distance. Supposing the corpuscular attraction equable throughout the whole sphere of its action, the aggregate cohesion of the successive parts of the stratum will be represented by the ordinates of a parabolic curve; for at any distance x from the surface, the whole interval being a, the fluxion of the force will be as $\dot{x}\ (a - x)$, since a number of particles proportional to $\dot{x}$ will be drawn downwards by a number proportional to a, and upwards by a number propor-

tional to x, and the whole cohesion, at the given point, will be expressed by $ax - \frac{1}{2}x^2$; and this at last becomes $\frac{1}{2}a^2$, which must be equal to the undiminished cohesion in the direction of the surface: consequently the difference of the forces acting on the sides of the elementary cube will everywhere be as $\frac{1}{2}a^2 - ax + \frac{1}{2}x^2$, and the fluxion of the whole contractile force will be $dx\ (\frac{1}{2}a^2 - ax + \frac{1}{2}x^2)$, the fluent of which, when $x = a$, becomes $\frac{1}{3}a^3$, which is one-third of $a \times \frac{1}{2}a^2$, the whole undiminished cohesion of the stratum.

We may therefore conclude, in general, that *the contractile force is one third of the whole cohesive force of a stratum of particles, equal in thickness to the interval to which the primitive equable cohesion extends;* and if the cohesive force be not equable, we may take the interval which represents its *mean* extent, as affording a result almost equally accurate. In the case of water, the tension of each inch of the surface is somewhat less than three grains; consequently we may consider the whole cohesive and repulsive force of the superficial stratum as equal to about nine grains. Now since there is reason to suppose the corpuscular forces of a section of a square inch of water to be equivalent to the weight of a column about 750,000 feet high, at least if we allow the cohesion to be independent of the density, their magnitude will be expressed by $252.5 \times 750{,}000 \times 12$ grains, which is to 9 as $252.5 \times 1000{,}000$ to 1; consequently the extent of the cohesive force must be limited to about the 250 millionth of an inch: nor is it very probable that any error in the suppositions adopted can possibly have so far invalidated this result as to have made it very many times greater or less than the truth.

Within similar limits of uncertainty, we may obtain something like a conjectural estimate of the mutual distance of the particles of vapours, and even of the actual magnitude of the elementary atoms of liquids, as supposed to be nearly in contact with each other; for if the distance, at which the force of cohesion begins, is constant at the same temperature, and if the particles of steam are condensed when they approach within this distance, it follows that at 60° of Fahrenheit the distance of the particles of pure aqueous vapour is about the 250

millionth of an inch; and since the density of this vapour is about one-sixty thousandth of that of water, the distance of the particles must be about forty times as great; consequently the mutual distance of the particles of water must be about the ten thousandth millionth of an inch. It is true that the result of this calculation will differ considerably according to the temperature of the substances compared; for the phenomena of capillary action, which depend on the superficial tension, vary much less with the temperature than the density of vapour at the point of precipitation: thus an elevation of temperature, amounting to a degree of Fahrenheit, lessens the force of elasticity about one ten-thousandth, the superficial tension about one thousandth, and the distance of the particles at the point of deposition about a hundredth. This discordance does not, however, wholly invalidate the general tenor of the conclusion; nor will the diversity resulting from it be greater than that of the actual measurements of many minute objects, as reported by different observers: for example those of the red particles of blood, the diameter of which may be considered as about two million times as great as that of the elementary particles of water, so that each would contain eight or ten trillions of particles of water, at the utmost. If we supposed the excess of the repulsive force of liquids above that of elastic fluids to depend rather on a variation of the law of the force than of the number of particles co-operating with each other, the extent of the force of cohesion would only be reduced to about two-thirds; and on the whole it appears tolerably safe to conclude, that, whatever errors may have affected the determination, the diameter or distance of the particles of water is between the two thousand and the ten thousand millionth of an inch.

Section II.—*Relations of Heterogeneous Substances.*

We must now return from this conjectural digression to the regions of strict mathematical argument, and inquire into the effect of the contact of substances of different kinds on the tension of their common surfaces, and on the conditions required for their equilibrium. Whatever doubts there may be respecting the variation of the number of particles co-operating when

the actual density of the substance is changed, there can be none respecting the consequence of the contact of two similar substances of different densities; for the less dense must necessarily neutralize the effects of an equivalent portion of the particles of the more dense, so as to prevent their being concerned in producing any contractility in the common surface, and the remainder, acting at the same interval as when the substance remained single, must obviously produce an effect proportional to the square of the number of particles concerned, that is, of the difference of the densities of the substances. This effect may be experimentally illustrated by introducing a minute quantity of oil on the surface of the water contained in a capillary tube, the joint elevation, instead of being increased as it ought to be according to Mr. Laplace, is very conspicuously diminished; and it is obvious that since the capillary powers are represented by the squares of the density of oil and of its difference from that of water, their sum must be less than the capillary power of water, which is proportional to the square of the sum of the separate quantities.

Upon these principles we may determine the conditions of equilibrium of several different substances meeting in the same point, neglecting for a moment the consideration of solidity or fluidity, as well as that of gravitation, in estimating the contractile powers of the surfaces, and their angular situations. We suppose then three liquids of which the densities are, A, B, and C, to meet in a line situated in the plane termination of the first, the contractile forces of the surfaces will then be expressed by $(A—B)^2$, $(A—C)^2$, and $(B—C)^2$; and if these liquids be so arranged as to hold each other in equilibrium, whether with or without the assistance of any external force, the equilibrium will not be destroyed by the congelation of the first of the liquids, so that it may constitute a solid. Now, unless the joint surface of the second and third coincides in direction with that of the first, it cannot be held in equilibrium by the contractility of this surface alone: but supposing these two forces to be so combined as to produce a result perpendicular to the surface of the first substance, this force may be resisted by its direct attraction, the forces which tend to cause

the oblique surface to move either way on it, balancing each other, and the perpendicular attraction being counteracted by some external force holding the solid in its situation: consequently the force expressed by $(B—C)^2$, reduced in the proportion of the radius to the cosine of the angle, must become equal to the difference of the forces $(A—B)^2$ and $(A—C)^2$, and if the radius be called unity, this cosine must be $\frac{(A-C)^2-(A-B)^2}{(B-C)^2} = \frac{2AB-2AC-(B^2-C^2)}{(B-C)^2} = \frac{2A-(B+C)}{B-C}$, which is the excess of twice the density of the solid above the sum of the densities of the liquids, divided by the difference of these densities; and when there is only one liquid, and $C = 0$, this cosine becomes $\frac{2A}{B} - 1$, vanishing when $2A = B$, and the density of the solid is half of that of the liquid, the angle then becoming a right one, as Clairaut long ago inferred from other considerations. Supposing the attractive density of the solid to be very small, the cosine will approach to —1, and the angle of the liquid to two right angles; and on the other hand, when A becomes equal to B, the cosine will be 1, and the angle will be evanescent, the surface of the liquid coinciding in direction with that of the solid. If the density A be still further increased, the angle cannot undergo any further alteration, and the excess of force will only tend to spread the liquid more rapidly on the solid, so that a thin film would always be found on its surface, unless it were removed by evaporation, or unless its formation were prevented by some unknown circumstance which seems to lessen the intimate nature of the contact of liquids with dry solids. For the case of glass and mercury we find $\frac{A}{B}$ about $\frac{1}{3}$, and the cosine $-\frac{3}{4}$, which corresponds to an angle of 139°; and if we add a second liquid, the expression will become $\frac{-6-C}{8-C}$, which will always indicate an angle less than 180°, as long as C remains less than 1, or as long as the liquid added is less dense than glass. There must, therefore, have been a slight inaccuracy in the observation mentioned by Mr. Laplace, that the surface of mercury contained in a glass tube becomes hemispherical under water: and if we could obtain an exact measurement of the angle assumed by the mercury under

these circumstances, we should at once be able to infer from it the comparative attractive density of water and glass, which has not yet been ascertained; although it might be deduced with equal ease from the comparative height of a portion of mercury, contained in two unequal branches of the same tube, observed in the air and under water. The cosine is more exactly $-.735$, in the case of the contact of glass and mercury, and $\frac{2A}{B} = 265$, whence $\frac{A}{B} = \frac{1}{7.55}$, which is a disproportion somewhat greater than that of the specific gravities, but it must probably vary with the various kinds of glass employed.

There is also another mode of determining the angle of contact of a solid with a single liquid, which has been ingeniously suggested by Mr. Laplace; it is derived from the principle of the invariability of the curvature of the surface at a given elevation; and its results agree with those which we have already obtained, except that it does not appear to be applicable to the case of more than one liquid in contact with the given solid. Supposing a capillary tube to be partially inserted into a liquid, if we imagine it to be continued into a similar tube of the liquid, leaving a cylinder or column of indefinite length in the common cavity; then the action of either tube, upon the liquid immediately within it, will have no tendency either to elevate or to depress the column: but the attraction of the portion of the tube above the column will tend to raise it with a certain force, and the lower end of the tube will exert an equal force upon the portion of the column immediately below it; and this double force will only be opposed by the single attraction of the liquid continuation of the tube, drawing down the column above it, so that the weight of the column suspended will be as the excess of twice the attractive force of the solid above that of the liquid. Now supposing two plates of the solid in question to approach very near each other, so that the elevation may be very great in comparison with the radius of curvature of the surface, which in this case may be considered as uniform; the weight suspended will then be simply as the elevation, which will be the measure of the efficient attractive force, and will vary with it, if we suppose the nature of the

solid to vary, the radius of curvature varying in the inverse ratio of the elevation: but the radius of curvature is to half the distance of the plates, as unity to the numerical sine of half the angular extent of the surface, or the cosine of the angle of the liquid, so that this cosine will be inversely as the radius, or directly as the elevation; that is, as the efficient attractive force, which is expressed by $2A - B$, becoming $= -1$ when A vanishes, and consequently being always equal to $\frac{2A - B}{B}$, as we have already found from other considerations. If we wished to extend this mode of reasoning to the effect of a repulsive force counteracting the cohesion, we should only have to suppose the diameter of the tube diminished on each side by the interval which is the limit of the repulsion, since beyond this the repulsion could not interfere with the truth of the conclusions, for want of any particles situated in the given directions near enough to each other to exhibit it; and within the stratum more immediately in contact with the solid, the forces may be supposed to balance each other by continuing their action along its surface until they are opposed by similar forces on the outside of the tube or elsewhere: and indeed such a repulsive stratum seems in many cases to be required for affording a support to the extended surface of the liquid when the solid does not project beyond it. It may also be shown, in a manner nearly similar, by supposing the column to be divided into concentric cylinders, that the superficial curvature of the liquid will not affect the truth of the conclusion.

Section III.—*Forms of Surfaces of Simple Curvature.*

We may now proceed upon the principle, admitted by all parties, of a hydrostatic pressure proportional to the curvature of the surface of the liquid, which is equivalent to a uniform tension of that surface, and which either supports the weight or pressure of the fluid within its concavity, or suspends an equal column from its convexity, whether with the assistance of the pressure of the atmosphere, or more simply, by the immediate effect of the same cohesion, that is capable of retaining the mercury of the barometer in contact with the summit

of the tube: and on this foundation, we may investigate the properties of the forms assumed by the surface; first considering the cases of simple curvature, which are analogous to some of the varieties of the elastic curve, and next those of the surfaces having an axis of revolution, which will necessarily involve us in still more complicated calculations.

A. *Let the height of the curve at its origin be a, the horizontal abscis x, the vertical ordinate y, the sine of the angular elevation of the surface s, the versed sine v, and the rectangle contained by the ordinate and the radius of simple curvature r; then the area of the curve will be rs, and* $y = \sqrt{(a^2 + 2rv)}$.

The fluxion of the curve z is jointly as the radius of curvature $\frac{r}{y}$, and as the fluxion of the angle of elevation, which we may call w, or $dz = \frac{r}{y}\,dw$, and $dx = \sqrt{(1 - s^2)}\,dz = \sqrt{(1 - s^2)}\frac{r}{y}\,dw$; but $\sqrt{(1 - s^2)}\,dw = ds$, consequently $dx = \frac{r}{y}\,ds$, and $y dx$, the fluxion of the area, becomes equal to $r ds$, and the area itself to rs. In order to find y, we have $dy = s dz = s\frac{r}{y}\,dw = \frac{r}{y}\,dv$; whence $y dy = r dv$, and $y^2 = 2rv + aa$, y becoming equal to a when v vanishes.

It may also be immediately inferred, that the area of the curve must vary as the sine of the inclination of the surface, from considering that, according to the principles of the resolution of forces, the tension being uniform, the weight which it supports must be proportional to that sine.

Scholium. The value of r for water at common temperatures is about one hundredth of a square inch, according to the results of a variety of experiments compared by Dr. Young; or more correctly, if we adopt the more recent measurement of Mr. Gay-Lussac, .0115: for alcohol Mr. Gay-Lussac's experiments give $r = .0047$; and for mercury $r = .0051$. Dr. Young had employed .005 for mercury, a number which appears to be so near the truth, that it may still be retained for the greater convenience of calculation. Hence in a very wide vessel, the smallest ordinate a being supposed evanescent, and $y = \sqrt{(2rv)} = .1516\sqrt{v}$, the height of the water rising against

the side of the vessel, when $v = 1$, will be .1516; and the utmost height at which the water will adhere to a horizontal surface, raised above its general level, will be $2\sqrt{r} = .2145$. For mercury, y becomes in these circumstances, $\sqrt{(.0102v)} = .101\sqrt{v}$, and if $s = .735$, $v = .322$, and the depression of the surface in contact with a vertical surface of glass becomes .0573; and again when $v = 1.735$, as in the case of a large portion of mercury lying on a plate of glass, the height y is .133: and if the glass had no attraction at all for mercury, v would become 2, and the height .1428. The actual tension of the surface of mercury is to that of water as $.0051 \times 13.6$ or .06936 to .0115; that is a little more than six times as great; while the angle of contact of mercury with glass, which is more attractive than water, would have led us to expect a disproportion somewhat greater. If we take a mean of these results, and estimate it at seven times, the value of $\sqrt{r}$ will be reduced, by immersing mercury standing on glass into water, in the ratio of $\frac{6}{7} \times \sqrt{\frac{13.6}{12.6}}$, since the buoyant effect of the water increases the value of r, so that $\sqrt{(2r)}$ will be .09; and the angle approaching to 180°, the height will be about .127.

B. *When the curve is infinite the abscis x becomes* $= \frac{1}{2}\sqrt{r}\,\mathrm{HL}\,\frac{2\sqrt{r} - \sqrt{(4r - yy)}}{2\sqrt{r} + \sqrt{(4r - yy)}} + \sqrt{(4r - y^2)}$, *reckoning from the greatest ordinate* $y = 2\sqrt{r}$; *and the excess of the length of the curve above the abscis is* $2\sqrt{r} - \sqrt{(4r - y^2)}$.

In this case, a being $= 0$, $y^2 = 2rv$: but $\frac{dx}{dy} = \frac{1 - v}{s} = \frac{1 - v}{\sqrt{(2v - vv)}} = \frac{2r - 2rv}{\sqrt{(4r - 2rv)}\sqrt{(2rv)}} = \frac{(2r - yy)}{\sqrt{(4r - yy)}\,y}$; and by the common rules for finding fluents, $x = \frac{2r}{4\sqrt{r}}\,\mathrm{HL}\,\frac{2\sqrt{r} - \sqrt{(4r - yy)}}{2\sqrt{r} + \sqrt{(4r - yy)}} + \sqrt{(4r - y^2)}$; which vanishes when $y = 2\sqrt{r}$: and for the length of the curve, since $\frac{dz}{dy} = \frac{1}{s} = \frac{1}{\sqrt{(2v - vv)}} = \frac{2r}{\sqrt{(4r - yy)}\,y}$, subtracting the former fluxional coefficient from this, we have $\frac{ydy}{\sqrt{(4r - yy)}}$ for the fluxion of the difference; and the fluent of this is $-\sqrt{(4r - y^2)}$.

Corollary 1. Hence, where the curve is vertical, we find $x = .5328\sqrt{r}$: and where the inclination amounts to a second, $x = 11.28\sqrt{r}$; for example, in the case of water, $\sqrt{r}$ being .1072, the latter value of x will become 1.21, and the former .056: so that the surface must be considered as sensibly inclined to the horizon at the distance of more than an inch from the vessel, but scarcely at an inch and a half: and for mercury, these distances will be two-thirds as great. This circumstance must not be forgotten when mercury is employed for an artificial horizon; although, where the vessel is circular, the surface becomes horizontal at its centre; and in other parts the inclination is materially affected by the double curvature.

Corollary 2. The form of the surface coincides in this case with that of an elastic bar, or a slender spring, of infinite length, supposed to be bent by a weight fixed to its extremity; since the curvature of such a spring must always be proportional to its distance from the vertical line passing through the weight. We may therefore deduce from this proposition the correction required for the length of a pendulum like Mr. Whitehurst's, consisting of a heavy ball, suspended by a very fine wire. Now the radius of curvature of the spring is $\frac{Maa}{12fy}$ (Art. BRIDGE, Prop. G.*); the modulus of elasticity, of which M is the weight, being for iron or steel about 10,000,000 feet in height; and since 80 inches of the wire weighed 3 grains, the thickness a, supposing it to have been $\frac{1}{3}$ or $\frac{2}{3}$ of the breadth, as is usual in wire flattened for hair springs, must have been about $\frac{1}{375}$ of an inch: the weight f was 12251 grains: and the weight M of ten million feet must have been $\frac{3}{38} \times 12 \times 10000000$ grains; consequently $\frac{Maa}{12fy} = \frac{3 \times 10000000}{80 \times 12251 \times 375 \times 375\, y} = \frac{1000}{12251 \times 375\, y} = \frac{1}{4594y}$, which is analogous to $\frac{r}{y}$ in these propositions; consequently $\sqrt{r} = \frac{1}{68}$; and the whole value of $\sqrt{(4r - y^2)}$ from $y = 2\sqrt{r}$ to $y = 0$ is $\frac{1}{34}$ of an inch. Now supposing the spring to have

* In the Supplement to the Encyclopædia Britannica, reprinted in the second volume of this work, No. LII.—*Note by the Editor.*

been firmly fixed at the axis of vibration, the excess of its length above the ordinate will always be measured by $2\sqrt{r} - \sqrt{(4r-y^2)}$; but $\sqrt{(4r-y^2)} = \sqrt{(4r-2rv)} = \sqrt{r}\sqrt{(4-2v)}$, which is the chord of the supplement of the arc of vibration in the circle of which the radius is $\sqrt{r} = \frac{1}{6}$; and the ball will be drawn above its path to a height equal to the distance between this circle and another of twice the diameter, touching it at its lowest point: but a perpendicular falling from this point on the wire would always be found in a circle twice as much curved as the first circle, and if it were made the centre of vibration, the ball would always be raised twice as far above its original path as the distance between the first circle and the second, which is the measure of the effect of the curvature; so that the pendulum must be supposed to be shortened half as much as this; that is, in the present instance, $\frac{1}{72}$ of an inch. If however the spring remained, in Mr. Whitehurst's experiments, at liberty to turn within the clip, and was firmly fixed at a considerable distance above, the variation of the length must have been only that which belongs to half of the arc of vibration; that is, one fourth as great as in the former case, since the versed sine is initially as the square of the arc; but since it would affect the spring both above and below the clip, it would be doubled from this cause, and would amount to $\frac{1}{36}$ of an inch: so that the true correction would be liable to vary from .00735 to .00367, according to the mode of fixing the wire. But since this error must have affected both Mr. Whitehurst's pendulums in an equal degree, and the result was deduced from the difference, and not the proportion of the lengths, it is free from any inaccuracy on this account. The calculation however sufficiently proves the necessity of attending to the effect of different modes of fixing the spring, in order that no variation may be made in the different experiments compared without a proper correction. The elasticity of such a wire as Mr. Whitehurst employed, could not have produced any sensible error, by co-operating with the force of gravitation, since it did not amount to one two-millionth part of the weight of the ball.

C. *The relation of the ordinate and absciss may be generally expressed by means of an infinite series.*

When the curve is concave towards the absciss throughout its extent, the ordinate may be compared with the lengths of hyperbolic and elliptic arcs, as Maclaurin has shown with respect to the elastic curve (*Fluxions*, § 928): but his solution fails in the more ordinary cases of the problem; and even where it is applicable, the calculation is very little facilitated by it. Segner has made use of two different forms of infinite series, each having its peculiar advantages with respect to convergence in particular cases, and other forms may be found, which will sometimes be more convenient than either of these. The value of the cotangent $\frac{dx}{dy}$ being in general

$$\frac{1-v}{\sqrt{(2v-vv)}} = \frac{2r-2rv}{\sqrt{(4r-2rv)}\sqrt{(2rv)}} = \frac{2r-yy+aa}{\sqrt{(4r-yy+aa)}} \cdot \frac{1}{\sqrt{(yy-aa)}},$$

we may retain either of these fractions, and expand the other by means of the binomial theorem.

1. In the first place, making $4r+a^2=c^2$, we have $(c^2-y^2)^{-\frac{1}{2}} = \frac{1}{c} + \frac{1}{2}\cdot\frac{y^2}{c^3} + \frac{3}{4.2}\cdot\frac{y^4}{c^5} + \frac{3.5}{8.2.3}\cdot\frac{y^6}{c^7} + \ldots$, and $\frac{dx}{dy} = \frac{2r+aa}{\sqrt{(yy-aa)}} \cdot \left(\frac{1}{c} + \frac{1}{2}\cdot\frac{y^2}{c^3} + \frac{3}{4.2}\cdot\frac{y^4}{c^5} + \ldots\right) - \frac{1}{\sqrt{(yy-aa)}} \cdot \left(\frac{y^2}{c} + \frac{1}{2}\cdot\frac{y^4}{c^3} + \frac{3}{4.2}\cdot\frac{y^6}{c^5} + \ldots\right)$. Now, in order to find the fluents of the separate terms, we have first $\int \frac{dy}{\sqrt{(yy-aa)}} = \text{HL}\,(y+\sqrt{[yy-aa]})$; and calling this logarithm L,

$$\int y^2 \sqrt{\frac{dy}{(yy-aa)}} = \frac{y}{2}\sqrt{(y^2-a^2)} + \frac{a^2}{2}\,\text{L};$$

$$\int y^4 \sqrt{\frac{dy}{(yy-aa)}} = \left(\frac{y^3}{4} - \frac{3a^2y}{8}\right)\sqrt{(y^2-a^2)} + \frac{3a^4}{8}\,\text{L};$$

$$\int y^6 \sqrt{\frac{dy}{(yy-aa)}} = \left(\frac{y^5}{6} - \frac{5a^2y^3}{24} + \frac{5a^4y}{16}\right)\sqrt{(y^2-a^2)} + \frac{5a^6}{16}\,\text{L}; \text{ and}$$

$$\int y^8 \sqrt{\frac{dy}{(yy-aa)}} = \left(\frac{y^7}{8} - \frac{7a^2y^5}{8.6} + \frac{7.5a^4y^3}{8.6.4} - \frac{7.5.3a^6y}{8.6.4.2}\right)\sqrt{(y^2-a^2)} + \frac{7.5.3a^8}{8.6.4.2}\,\text{L}:$$

whence by substitution we have

$$x = \frac{2r+aa}{4r+aa}\,\text{L} + \left(\frac{2r+aa}{2(4r+aa)^{\frac{3}{2}}} - \frac{1}{4r+aa}\right)\cdot\left(\frac{y}{2}\sqrt{[y^2-a^2]} + \frac{a^2}{2}\,\text{L}\right) + \ldots.$$

2. If we reduce $\frac{1}{\sqrt{(yy-aa)}}$ into a series, we have $\left(1-\frac{aa}{yy}\right)^{-\frac{1}{2}} = 1 + \frac{1}{2}\frac{a^4}{y^2} + \frac{3}{4.2}\cdot\frac{a^4}{y^4} + \frac{3.5}{8.2.3}\cdot\frac{a^6}{y^6} + \ldots$, and $\frac{dx}{dy} = \frac{2r+aa-yy}{\sqrt{(cc-yy)}}\cdot\left(\frac{1}{y} + \frac{1}{2}\cdot\frac{a^2}{y^2} + \frac{3}{4.2}\cdot\frac{a^4}{y^5} + \ldots\right)$. Then, for the fluents,

$$\int \frac{ydy}{\sqrt{(cc-yy)}} = \sqrt{(c^2-y^2)}\,;\ \int \frac{dy}{y\sqrt{(cc-yy)}} = \mathrm{HL}\frac{c-\sqrt{(cc-yy)}}{c+\sqrt{(cc-yy)}} = \mathrm{L}'\,;$$

$$\int \frac{dy}{y^3\sqrt{(cc-yy)}} = -\frac{\sqrt{(cc-yy)}}{2ccyy} - \frac{1}{2cc}\mathrm{L}'\,;$$

$$\int \frac{dy}{y^5\sqrt{(cc-yy)}} = \left(-\frac{1}{4c^2y^4} + \frac{3}{8c^4y^2}\right)\sqrt{(c^2-y^2)} + \frac{3}{8c^4}\mathrm{L}'\,;$$

$$\int \frac{dy}{y^7\sqrt{(cc-yy)}} = \left(-\frac{1}{6c^2y^6} + \frac{5}{24c^4y^4} - \frac{5}{16c^6y^2}\right)\sqrt{(c^2-y^2)} - \frac{5}{16c^6}\mathrm{L}'\,;$$

and by combining these fluents we obtain a second series for x.

3. These series may be employed with advantage where the initial ordinate is very small, the one being more convenient for the upper, and the other for the lower part of the curve: but where the elevation a is more considerable, the form of the curve will be more readily determined by means of fluents derived from circular arcs. Beginning with the expressions $\frac{dx}{dy} = \sqrt{\frac{1-v}{(2v-vv)}}$, and $y^2 = a^2 + 2rv$, we may seek for a value of x in terms of v; and since $2ydy = 2rdv$, $dy = \frac{r}{y}dv = \sqrt{\frac{rdv}{(aa+2rv)}}$, and $dx = \sqrt{\frac{1-v}{(2v-vv)}}\cdot\sqrt{\frac{rdv}{(aa+2vo)}}$. The binomial $(aa+2rv)^{-\frac{1}{2}}$ may then be expanded into a series of integral powers of v, and the fluents may be found by means of the equations $\int \sqrt{\frac{dv}{(2v-vv)}} = \int \frac{dv}{s} = w$, the arc of which v is the versed sine; $\int \frac{vdv}{s} = s - w$; $\int \frac{v^2dv}{s} = \left(\frac{v}{2} - \frac{3}{4}\cdot 2\right)s + \frac{3}{8}\cdot 4w$; $\int \frac{v^3dv}{s} = \left(\frac{v^2}{3} - \frac{5}{24}\cdot 4v + \frac{5}{16}\cdot 8\right)s - \frac{5}{16}\cdot 8w$; and $\int \frac{v^4dv}{s} = \left(\frac{v^3}{4} - \frac{7}{8.6}\cdot 4v^2 + \frac{7.5}{8.6.4}\cdot 8v - \frac{7.5.3}{8.6.4.2}\cdot 16\right)s + \frac{7.5.3}{8.6.4.2}\cdot 16w$.

4. Another series may be obtained by the expansion of $\frac{1}{\sqrt{(2v - vv)}}$ into $\frac{1}{\sqrt{(2v)}} \cdot \left(1 + \frac{1}{4} v + \frac{3}{8.2} v^2 + \frac{5}{16.2.3} v^3 + \ldots\right)$, whence $\frac{dx}{dy} = \left(1 - \frac{yy - aa}{2r}\right) \sqrt{\frac{r}{yy - aa}} \left(1 + \frac{1}{4} \frac{yy - aa}{2r} + \frac{3}{8.2} \frac{(yy-aa)^2}{2r} + \ldots\right)$: the fluxions belonging to the series $(y^2 - a^2)^{-\frac{1}{2}} dy$, $(y^2 - a^2)^{\frac{1}{2}} dy$, $(y^2 - a^2)^{\frac{3}{2}} dy$; and the fluents of these are $HL\ (y + \sqrt{[y^2 - a^2]}) = L$; $\frac{1}{2} y \sqrt{(y^2 - a^2)} - \frac{1}{2} a^2 L$; $(\frac{1}{4} \sqrt{(y^2 - a^2)} + \frac{3}{8} a^2) y \sqrt{(y^2 - a^2)} + \frac{3a^4}{8} L$; which afford a result somewhat resembling that which is deduced from the first method.

5. We may also express x in a series of integral powers of y only, if we suppose it to begin at some point in which the curve is inclined to the horizon, where the height is p, calling it at other points $p + y$; and making $\frac{dx}{dy} = \tau = a + by + cy^2 + \ldots$; we have then $x = \beta + ay + \frac{1}{2} by^2 + \frac{1}{3} cy^3 + \ldots$, and the area $\int (p + y)\, dx = \gamma + pay + \frac{1}{2} pby^2 + \ldots + \frac{1}{2} ay^2 + \frac{1}{2.3} by^3 + \frac{1}{3.4} cy^4 + \ldots$, which must be equal to rs (Prop. A.): but $s = \frac{dy}{\sqrt{(dx^2 + dy^2)}} = \frac{1}{\sqrt{(1 + \tau\tau)}}$, which may be developed by means of the Taylorian theorem $\varphi (A + H) = \varphi A + \frac{d(\varphi A)}{dA} H + \frac{(d^2 \varphi A)}{dA^2} \cdot \frac{H^2}{2} + \ldots$, taking $A = a$, and $H = by + cy^2 + \ldots$, whence

$H^2 = b^2y^2 + 2bcy^3 + (2bd + c^2)\, y^4 + \ldots$

$H^3 = b^3y^3 + 3b^2cy^4 + \ldots$; consequently $rs = r\varphi a \ldots$

$$= \frac{r}{\sqrt{(1 + aa)}} + \frac{r}{da} \cdot d \frac{1}{\sqrt{(1 + aa)}} (by + cy^2 + dy^3 + ey^4 + \ldots)$$
$$+ \frac{r}{2da^2} \cdot d^2 \frac{1}{\sqrt{(1 + aa)}} \cdot (b^2y^2 + 2bcy^3 + (2bd + c^2) \cdot y^4 + \ldots)$$
$$+ \frac{r}{2.3da^3} \cdot d^3 \frac{1}{\sqrt{(1 + aa)}} (b^3y^3 + 3b^2cy^4 + \ldots) =$$
$$\gamma + pay + (\tfrac{1}{2}pb + \tfrac{1}{2}a) y^2 + (\tfrac{1}{3}pc + \tfrac{1}{6}b) y^3 + \ldots;$$

and hence by comparing the homologous terms, we find $\gamma = \frac{r}{\sqrt{(1+aa)}}$, $\frac{n}{da} \cdot d\frac{1}{\sqrt{(1+aa)}} \cdot b = pa = \frac{-ra}{(1+aa)^{\frac{3}{2}}} \cdot b$, and $b = -\frac{p}{r}(1+aa)^{\frac{3}{2}}$; and in a similar manner we may determine the subsequent coefficients; but the calculation is somewhat laborious, and has no particular advantages.

6. We may still more readily obtain a similar series for y in terms of the powers of x with constant coefficients; calling $\frac{dy}{dx}$, t, and making $t = bx + cx^3 + dx^5 + \ldots$, whence $y - a + \frac{1}{2}bx^2 + \frac{1}{4}cx^4 + \frac{1}{6}dx^6 + \ldots$, and the area $\int y dx = ax + \frac{1}{2.3}bx^3 + \frac{1}{4.5}cx^5 + \ldots = rs = \frac{rt}{\sqrt{(1+tt)}} = rt\,(1 - \frac{1}{2}t^2 + \frac{3}{4.2}t^4 - \frac{3.5}{8.2.3}t^6 + \ldots)$. But $t^3 = b^3x^3 + 3b^2cx^5 + \ldots$ and $t^5 = b^5x^5 + \ldots$: hence we have the equation

$$\begin{array}{llll} \frac{ax}{r} + \frac{1}{2.3r}bx^3 + \frac{1}{4.5r}cx^5 & + \ldots = & & \\ bx + cx^3 & + dx^5 & + \ldots & \\ -\frac{1}{2}b^3x^3 & -\frac{1}{2}.3b^2cx^5 & - \ldots & \\ & +\frac{3}{8}b^5x^5 & + \ldots : \text{ consequently} & \end{array}$$

$$b = \frac{a}{r},\ c = \frac{1}{2.3r}b + \tfrac{1}{2}b^3, \text{ and } d = \frac{1}{4.5r}c + \tfrac{3}{2}b^2c - \tfrac{3}{8}b^5.$$

It is the less necessary to enter into any further detail of these results, as we have a table, calculated by Segner, with his son's assistance, which is sufficient to afford us a general idea of the forms of the curve in different circumstances. The unit of this table is the quantity $\sqrt{r}$, which Segner calls the modulus of capillary attraction, and which for water is .1072 inch. The table begins with the extreme ordinate, where the curve is vertical: we have then the least ordinate, a; the greatest ordinate, where the curve again becomes horizontal, and the abscis corresponding to the extreme ordinate and to the greatest ordinate.

Extreme Ordinate.	Least Ordinate.	Greatest Ordinate.	Greatest Absciss.	Terminal Absciss.
$100\sqrt{r}$	99.99	100.01	.01	.000001
90	89.99	90.01	.01	.000002
80	79.99	80.01	.01	.000003
70	69.99	70.01	.01	.000004
60	59.99	60.02	.02	.000007
50	49.98	50.02	.02	.00001
45	44.98	45.02	.02	.00002
40	39.97	40.02	.02	.00003
35	34.97	35.03	.03	.00004
30	29.96	30.03	.03	.00006
25	24.96	25.04	.04	.0001
20	19.95	20.05	.05	.0002
15	14.93	15.07	.07	.0004
10	9.90	10.10	.10	.001
9	8.89	9.11	.11	.002
8	7.87	8.12	.13	.003
7	6.85	7.14	.14	.004
6	5.83	6.16	.17	.007
5	4.79	5.19	.21	.01
4	3.74	4.24	.26	.02
3	2.64	3.32	.37	.06
2	1.41	2.45	.65	.22
1.9	1.27	2.37	.71	.27
1.8	1.11	2.29	.79	.33
1.7	.94	2.21	.91	.47
1.6	.75	2.13	1.10	.65
1.5	.50	2.06	1.40	.96
1.47	.40	2.04	1.64	1.18
1.445	.30	2.02	1.86	1.44
1.428	.20	2.01	2.24	1.82
1.418	.10	2.003	2.92	2.49
1.4142	.01	2.000	5.22	4.80
1.4142	.001	2.0000	7.52	7.09
1.4142	.0001	2.0000	9.82	9.39
1.4142	.00001	2.0000	12.12	11.70
1.4142	.000001	2.0000	14.43	14.00

It may be observed that the last six values of the least ordinate are in geometrical progression, while the absciss increases in arithmetical progression; the difference of the abscisses 2, 3, being the hyperbolical logarithm of 10, which is the common multiplier of the ordinates. Although the table appears to be generally accurate, yet we cannot always depend on the last figures: thus the ultimate difference of the two last columns is made .43, while it ought to be .53 (Prop. B. Cor. 1). It is scarcely necessary to remark, that if we look, in the fourth column, for half the distance between two parallel planes of glass, in a vertical position, the first and second columns will give us the height to which water will rise between them, where it touches the glass, and in the middle of the interval.

Section IV.—*Surfaces of Double Curvature.*

When the liquid is contained in a tube, or when it forms itself spontaneously into a drop having an axis of revolution, it becomes necessary to consider the effect of the tension in a direction transverse to that of the principal section; since the curvature will cause it to exhibit an equal pressure, whatever the direction of the section to which it belongs may be; and the curvatures of the sections perpendicular to each other will either co-operate with, or counteract each other, accordingly as the convexities of both are on the same side, or on the opposite sides, of the surface. But the simple consideration of the tension, supporting the weight of the parts below, or the equivalent pressure in a contrary direction, will at once afford us the equations necessary for the solution of the problem, without any immediate reference to the curvature in question.

D. *The form of a surface of revolution may be determined by means of an infinite series.*

The fluxion of the weight or mass of the parts, contained within the cylindrical surface, of which x is the radius or absciss, and y the ordinate, being always proportional to $yx\dot{x}$, and the fluent to $\int yx\dot{x}$; and the extent of the circumference supporting it varying also as x, and the contractile force being diminished, when reduced to the direction of gravitation, in the ratio of the radius unity to the sine of the elevation s, it will always be proportional to xs; so that we have the general equation $\int yx\dot{x} = mxs$. Now if we suppose y incomparably greater than x, and the surface extremely minute, the variation of y may be neglected, and we have in this case $\int yx\dot{x} = \frac{1}{2}yx^2$: and supposing also $s = 1$, and the curve vertical, $\frac{1}{2}yx^2 = mx$, and $\frac{1}{2}\ yx = m$; x becoming also equal to the radius of curvature: but it is easy to perceive that the height y must be twice as great, for any value of x, as in the case of a simple curvature, since each portion of the circumference has here only to support a wedge, which is only half as heavy as a parallelepiped of the same

height; so that $\frac{1}{2}yx$ will be equal to yx in Proposition A, and $m = r$.

In order to obtain a series for finding y, from the equation $\int yx\mathrm{d}x = mxs$, we may put the tangent $t = \frac{\mathrm{d}y}{\mathrm{d}x} = bx + cx^3 + dx^5 + \ldots$, whence $y = a + \frac{1}{2}bx^2 + \frac{1}{4}cx^4 + \frac{1}{6}dx^6 + \ldots$, and $\int yx\mathrm{d}x = \frac{1}{2}ax^2 + \frac{1}{2.4}bx^4 + \frac{1}{4.6}cx^6 + \frac{1}{6.8}dx^8 + \ldots$; and the value of $s = \frac{t}{\sqrt{(1+tt)}}$ being expanded into a series, as in Proposition C, n. 6, calling $\frac{1}{m}$, or $\frac{1}{r}$, q, we find $s = \frac{q}{x}\int yx\mathrm{d}x$

$$= bx + cx^3 + dx^5 + ex^7 + \ldots$$
$$- \tfrac{1}{2}b^3x^3 - \tfrac{1}{2}\cdot 3b^2cx^5 - \tfrac{1}{2}\left\{\begin{matrix}3b^2d + \\ 3bc^2\end{matrix}\right\}x^7 + \ldots$$
$$+ \tfrac{3}{8}\ b\ x^5 \ + \tfrac{3}{8}\cdot 5b^4c\ x^7 + \ldots$$
$$- \ \tfrac{5}{1.6}\ b^7\ x^7 - \ldots$$

$= \frac{1}{2}qax + \frac{1}{2.4}qbx^3 + \frac{1}{4.6}qcx^5 + \frac{1}{6.8}qdx^7 + \ldots$; consequently $b = \frac{1}{2}qa = \frac{a}{2r}$, and $a = \frac{2b}{q} = 2rb$, and by continuing the calculation and reducing the values, we find

$$c = \frac{1}{2.4}qb + \tfrac{1}{2}b^3$$

$$d = \frac{1}{2.4^2.6}q^2b + \frac{10}{2.4.6}qb^3 + \frac{3}{2.4}b^5$$

$$e = \frac{1}{2.4^2.6^2.8}q^3b + \frac{82}{2.4.6^2.8}q^2b^3 + \frac{105}{2.4.6.8}qb^5 + \frac{15}{2.4.6}b^7$$

$$f = \frac{1}{2.4^2.6^2.8^2.10}q^4b + \frac{652}{2.4.6^2.8^2.10}q^3b^3 + \frac{2645}{2.4.6.8^2.10}q^2b^5 + \frac{1260}{2.4.6.8.10}qb^7 + \frac{105}{2.4.6.8}b^9$$

$$g = \frac{1}{2.4^2.6^2.8^2.10^2.10}q^5b + \frac{5197}{2.4.6^2.8^2.10^2.12}q^4b^3 + \frac{59355}{2.4.6.8^2.10^2.12}q^3b^5 + \frac{70522.5}{2.4.6.8.10^212}q^2b^7 + \frac{17325}{2.4.6.8.10.12}qb^9 + \frac{945}{2.4.6.8.10}b^{11}$$

$$h = \frac{1}{2.4^2.6^2..12^2.14}q^6b + \frac{41\ 8}{2.4.6^2.8^2..12^2.14}q^5b^3 + \frac{1303034}{2.4.6.8^2.10^2.12^2.14}q^4b^5 + \ldots$$

$$i = \frac{1}{2.4^2.6^2..14^2.16}q^7b + \frac{339412}{2.4.6^2.8^2..14^2.16}q^6b^3 + \ldots$$

$$k = \frac{1}{2.4^2.6^2..16^2.18}q^8b + \frac{2779888}{2.4.6^2.8^2..16^2.18}q^7b^3 + \ldots$$

$$l = \frac{1}{2.4^2.6^2..18^2.20.}q^9b + \frac{22941328}{2.4.6^2.8^2..18^2.20}q^8b^3 + \ldots$$

We may here observe, that the numerical coefficients of the highest powers of b form the series $\frac{1}{2}, \frac{3}{2.4}, \frac{3.5}{2.4.6}, \frac{3.5.7}{2.4.6.8}$, &c., the ratio of the successive terms of both continually approaching to equality; and those of the next in order, the series $\frac{3}{2.4} \cdot \frac{2}{6}, \frac{3.5}{2.4.6} \cdot \frac{4}{6}, \frac{3.5.7}{2.4.6.8} \cdot \frac{6}{6}, \frac{3.5.7.9}{2.4.6.8.10} \cdot \frac{8}{6}$, &c.: but the laws of the numerical coefficients in general appear to be wholly incapable of being reduced to any simple form. It will be convenient for calculation to form tables of the logarithmic values of these coefficients, which may be continued, by means of successive differences, for as many terms as are requisite for any practical purpose. The indices, with lines drawn over them, are to be considered as negative numbers.

Logarithmic Coefficients of the Value of the Sine.

$s = ($ 0. 0000000
$+ \overline{1}.\ 0969100\ qx^2$
$+ \overline{3}.\ 7166987\ q^2x^4$
$+ \overline{4}.\ 0354574\ q^3x^6$
$+ \overline{6}.\ 1323674\ q^4x^8$
$+ \overline{8}.\ 0531861\ q^5x^{10}$
$+ \overline{11}.\ 8278768\ q^6x^{12}$
$+ \overline{13}.\ 4776288\ q^7x^{14}$
$+ \overline{15}.\ 0182362\ q^8x^{16}$
$+ \overline{18}.\ 4619336\ q^9x^{18}$
$+ \overline{21}.\ 8184809\ q^{10}x^{20}$
$+ \ldots) bx$
$+ (\overline{2}.\ 3187587\ qx^2$
$+ \overline{3}.\ 6375174\ q^2x^4$
$+ \overline{4}.\ 6482413\ q^3x^6$
$+ \overline{5}.\ 4694937\ q^4x^8$
$+ \overline{6}.\ 1456895\ q^5x^{10}$
$+ \overline{8}.\ 7008651\ q^6x^{12}$
$+ \overline{9}.\ 1510234\ q^7x^{14}$
$+ \overline{11}.\ 5080209\ q^8x^{16}$
$+ \overline{13}.\ 7811595\ q^9x^{18}$
$+ \overline{15}.\ 9774\ldots\ q^{10}x^{20}$
$+ \overline{16}.\ 1026\ldots\ q^{11}x^{22}$
$+ \overline{18}.\ 160\ldots.\ q^{12}x^{24}$
$+ \overline{20}.\ 16\ldots.\ q^{13}x^{26}$
$+ \overline{22}.\ 10\ldots.\ q^{14}x^{28}$
$+ \overline{24}.\ 0\ldots.\ q^{15}x^{30}$
$+ \ldots) b^3x^3$
$+ (\overline{3}.\ 8927900\ qx^2$
$+ \overline{3}.\ 5337080\ q^2x^4$
$+ \overline{4}.\ 8558231\ q^3x^6$
$+ \overline{5}.\ 9851885\ q^4x^8$
$+ \overline{6}.\ 9727959\ q^5x^{10}$
$+ \overline{7}.\ 82\ldots.\ q^6x^{12}$
$+ \overline{8}.\ 57\ldots.\ q^7x^{14}$
$+ \overline{9}.\ 27\ldots.\ q^8x^{16}$
$+ \overline{11}.\ 97\ldots.\ q^9x^{18}$
$+ \overline{12}.\ 77\ldots.\ q^{10}x^{20}$
$+ \ldots) b^5x^5$
$+ (\overline{3}.\ 5917600\ qx^2$

$+ \overline{3}.\ 4368580\ q^2x^4$
$+ \overline{4}.\ 9595058\ q^3x^6$
$+ \ldots) b^7x^7$
$+ \overline{3}.\ 3576767\ qx^2$
$+ \overline{3}.\ 3498514\ q^2x^4$
$+ \ldots) b^9x^9$
$+ (\overline{3}.\ 1657913\ qx^2$
$+ \ldots) b^{11}x^{11}$
$+ \ldots$

Logarithmic Coefficients of the Value of the Ordinate y.

$y = \left[\frac{2b}{q}\right] + (0.\ 6989700$
$+ \overline{2}.\ 4948500\ qx^2$
$+ \overline{4}.\ 9385474\ q^2x^4$
$+ \overline{5}.\ 1323674\ q^3x^6$
$+ \overline{7}.\ 1323674\ q^4x^8$
$+ \overline{10}.\ 9740048\ q^5x^{10}$
$+ \overline{12}.\ 6817488\ q^6x^{12}$
$+ \overline{14}.\ 2735087\ q^7x^{14}$
$+ \overline{17}.\ 7629636\ q^8x^{16}$
$+ \overline{19}.\ 1609036\ q^9x^{18}$
$+ \ldots) bx^2$
$+ (\overline{1}.\ 0969100$
$+ \overline{2}.\ 5406074\ qx^2$
$+ \overline{3}.\ 6482413\ q^2x^4$
$+ \overline{4}.\ 5486749\ q^3x^6$
$+ \overline{5}.\ 2918175\ q^4x^8$
$+ \overline{7}.\ 9049851\ q^5x^{10}$
$+ \overline{8}.\ 4062959\ q^6x^{12}$
$+ \overline{10}.\ 8090509\ q^7x^{14}$
$+ \overline{11}.\ 1235822\ q^8x^{16}$
$+ \overline{13}.\ 3576\ldots\ q^9x^{18}$
$+ \overline{15}.\ 5176\ldots\ q^{10}x^{20}$
$+ \overline{17}.\ 607\ldots.\ q^{11}x^{22}$
$+ \overline{19}.\ 637\ldots.\ q^{12}x^{24}$
$+ \overline{21}.\ 60\ldots\ldots\ q^{13}x^{26}$
$+ \overline{23}.\ 5\ldots\ldots\ q^{14}x^{28}$
$+ \ldots) b^3x^4$
$+ (\overline{2}.\ 7958800$
$+ \overline{2}.\ 5337080\ qx^2$
$+ \overline{3}.\ 9350043\ q^2x^4$
$+ \overline{3}.\ 1313165\ q^3x^6$
$+ \overline{4}.\ 1769159\ q^4x^8$
$+ \overline{5}.\ 08\ldots\ldots\ q^5x^{10}$
$+ \overline{7}.\ 87\ldots\ldots\ q^6x^{12}$
$+ \overline{8}.\ 51\ldots\ldots\ q^7x^{14}$
$+ \overline{9}.\ 35\ldots\ldots\ q^8x^{16}$
$+ \overline{10}.\ 18\ldots\ldots\ q^9x^{18}$
$+ \ldots) b^5x^6$
$+ (\overline{2}.\ 5917600$
$+ \overline{2}.\ 6709412\ qx^2$
$+ \overline{2}.\ 1056338\ q^2x^4$
$+ \ldots) b^7x^8$
$+ (\overline{2}.\ 4368579$
$+ \overline{2}.\ 4959794\ qx^2$
$+ \ldots) b^9x^{10}$
$+ (\overline{2}.\ 3119193$
$+ \ldots) b^{11}x^{12}$
$+ \ldots$

The calculation is somewhat facilitated by obtaining the value of λy from the expansion of $s\sqrt{(1-s^2)^{-\frac{1}{2}}}$, and determining the coefficients of the series for s at once.—*MS. note by Dr. Young, Oct.* 8, 1818.

E. *The elevation or depression of a liquid contained in a given tube may be found by reversing the series.*

Having a given value of x, the semidiameter of the tube, and also of s, the elevation or depression of the surface of the liquid at the point of contact with the solid, we obtain an equation of the form $s = Ab + Bb^3 + Cb^5 + \ldots$, and from this we may determine the central elevation or depression $a = 2rb$ by the well known method of the reversion of series, which gives us the value $b = \frac{1}{A}s - \frac{B}{A^4}s^3 - \left(\frac{C}{A^6} - \frac{3B^2}{A}\right)s^5 - \left(\frac{D}{A^8} - \frac{8BC}{A^9} + \frac{12B^3}{A^{10}}\right)s^7 - \ldots$. But it is more convenient to assume an approximate value of b, a little less than $\frac{s}{A}$, and to find the corresponding value of s; then since $ds = Ad\,b + 3B\,b^2 db + 5C\,b^4 db + \ldots$, if we make $Ab + 3Bb^3 + 5Cb^5 + \ldots = \Sigma$, we shall have $\frac{ds}{db} = \frac{\Sigma}{b}$. consequently the small increments of s and b will be to each other as Σ to b, and we obtain the correction of b from the error of the calculated value of s: and if the calculation be repeated with the corrected value of b, the second result will always be sufficiently near to the truth.

In order to judge of the accuracy of this mode of calculation, which Mr. Laplace appears to have thought liable to some undefined objection, it will be necessary to enter into the details of its different elements, which will sufficiently show the degree of convergence of the series, and the greatest possible amount of error.

Values of the Coefficients of s for Tubes of different Diameters, r being .005, and s = .75.

$D = 2x$	$s = bx \times$	$+b^3x^3 \times$	$+b^5x^5 \times$	$+b^7x^7 \times$
1.0	47.176	7190		
.8	15.774	274		
.6	5.737	13.214	200	
.4	2.399	.8556	1.625	
.2	1.2717	.06311	.03693	.0311
.1	1.0638	.01155	.00486	.00278

Hence if

$b =$	$s =$
0.3073	.7248±.0252
.1147	.7237+.0265
.4160	.7160+.0254+.0060+[.0026]
1.503	.7211+.0240+.0041+[.0010]
5.776	.7345+.0122+.0024+ .0007 +[.0003]
14.004	.7449+.0040+.0008+ .0002 +[.0001]

It appears upon inspection of this table, that the coefficients of bx alone always determine $\frac{2}{3}\frac{9}{0}$ of the value of the quantity required, and these are easily calculated with perfect accuracy, so that the error must always be far less than $\frac{1}{3}\frac{}{0}$, and in fact the actual uncertainty never exceeds $\frac{1}{1}\frac{}{0}\frac{}{0}$ of the whole, at least in the last four examples. The differences of Mr. Laplace's approximatory calculations from these results are incomparably greater, so that we cannot hesitate to consider these differences as errors. Indeed, when we recollect that in the method employed by Mr. Bouvard, under Mr. Laplace's directions, the radius of curvature of each of the small portions, into which the curve has been cut up, has been determined from the ordinate at the beginning of the portion, it is obvious that the curvature thus found must be less than the truth, and that in order to obtain any required curvature of the whole surface the depression must be increased in the same proportion : and there is no ready way of appreciating the amount of this error. Dr. Young had before attempted to avoid it, in making an estimate of the same nature, by calculating for the middle of each portion; but from some accident, the numbers of his table, published in 1807, are generally a little too small, although the method, which he then employed, is nearly the same as that which Mr. Laplace afterwards adopted; except that for the lowest portion of the curve, Mr. Laplace had recourse to an infinite series, applicable only to that part. The elements deduced in Nicholson's Journal for 1809,* from Mr. Gay Lussac's experiments, which are $r = .0051$ and $s = .7353$, agree better with the numbers found in Mr. Laplace's table, than those from

* In an article contributed by Dr. Young.—*Note by the Editor.*

which it was constructed, which were $r = .005038$ and $s = .729$; the depressions being always a little larger than the true results from the elements assumed.

The value of the ordinate y depends also principally on the first variable member of the series, although the subsequent coefficients are not so inconsiderable as in the determination of the sine. Thus taking $x = .2$, and $b = 1.503$, we have $y = a + .813\ bx^2 + .99\ b^3x^4 + 2.97\ b^5x^6 + \ldots = .01503 + .0489 + .0054 + .0015 + [.0006] = .0714$, which is the marginal depression, leaving .0564 for the height of the convex portion $y - a$. We may determine the effect of any small variations in this height, in the same manner as that of the sine of the inclination: supposing them to depend on a change of the angle of contact only, the quantity r remaining unaltered, it is obvious that q and x must retain their value, while y and b only vary; and making $Y = Ab + 3Bb^3 + \ldots = b\frac{d(y-a)}{db}$, we have $Y : b = d(y-a) : db$. In the present instance, we find $Y = .0489 + 3 \times .0054 + 5 \times .0015 + \ldots = .070$; and supposing, as in the example suggested by Mr. Laplace, the variation of the height $y - a$ to be .00304, which is $\frac{1}{20}$ of Y, that of b will be $\frac{1}{20}$ of b, or .075, and the variation of the central depression a, .00075, which is somewhat less than one fifth of the alteration in the height of the convex portion; but in smaller tubes it is obvious that the variations of the depression a might much exceed that of the height of the convex portion. Nothing can be easier or more direct than this part of the calculation: and it is remarkable that Mr. Laplace should have considered the awkward contrivance of building up a curve, like the arch of a bridge, with fourteen blocks on each side, as possessing anything like an "advantage" over the series in the determination of this variation.

If we wish to find the effect of a small variation of the diameter of a tube, from D to $D \pm D'$, on the depression a of the mercury contained in it, we may use for the interpolation the formula $\frac{a'}{a} = 10^{\frac{CD'}{D}} - 1$, C being about 2.9 for tubes between 1 inch and $\frac{1}{4}$th of an inch in diameter, and being elsewhere easily deduced from the depressions already known. For variations of

the cohesive power, and of its measure r we may suppose the whole of the numbers of the table to be altered in the proportion of the supposed alteration of $\sqrt{r}$, and the change produced by restoring the diameter to its former dimensions may then be calculated like any other interpolation. There is also a more comprehensive formula, which seems to express the depression in tubes of all sizes with great accuracy: it is this, $a = \frac{.015}{D + 48\,D^{3.492 + 3.26\,D}}$: and it might even be possible to shorten the original calculation by a comparison of the series with the expansion of this empirical formula, if it were of any further importance to facilitate the mode of computation. But for all practical purposes, it will be sufficient to collect the results already obtained into a comparative table, arranged in chronological order: and it is remarkable, that they are all comprehended, without any material exception, between the two values assigned to each as near the truth in Dr. Young's first table, the mean of those values never differing a thousandth of an inch from the result of the more correct calculation; while the error of Lord Charles Cavendish's experiments, notwithstanding their general accuracy, sometimes amounts to nearly one hundredth.

Table of the Depression of Mercury in Glass Tubes.

DIAMETER.	CENTRAL DEPRESSION.											MARGINAL DEPRESSION. Nich., 1809.	DIFFERENCE.	DIAMETER.
	Observed by Lord Cavendish.	Dr. Young, Phil. Trans., 1805.		Laplace, 1800.	Dr. Young, 1807.	Nicholson's Journal, 1809.		Laplace, 1810.	Correct Calculation.		Empirical Formula.			
Inches.		Diagr.	Form.			$r=.005$ $s=.750$	$r=.0051$ $s=.7353$		$r=.005$ $s=.750$	$r=.005038$ $s=.729$		$r=.005$ $s=.750$		
1.00	..	..	..	..	..	.00031	.00032	..	.0003073	.000307	.00031	..	..	1.00
.90	..	..	..	..	..	.00060	.00062	..	..	..	.00059	..	..	.90
.80	..	..	..	..	..	.00115	.00118	.00128	.001147	.001144	.00112	..	..	.80
.70	..	..	..	..	..	.00220	.00224	.00244	..	..	.00220	..	..	.70
.60	.005	.005	..	.0038	.0045	.00411	.00416	.00462	.004160	.04128	.00421	.0637	.0596	.60
.50	.007	.007	.008	.0069	.0074	.00799	.00805	.00868	..	..	.Q0799	.0676	.0596	.50
.45	..	..	..	..	.0100	.01100	.01106	.01174	..	..	.01099	.0690	.0580	.45
.40	.015	.012	.017	.0136	.0139	.01516	.01522	.01591	.01503	.01486	.01495	.0714	.0562	.40
.35	.025	.017	.024	..	.0196	.02093	.02098	.02165	..	..	.02082	.0745	.0536	.35
.30	.036	.027	.033	..	.0280	.02902	.02906	.02965	..	..	.02881	.0787	.0497	.30
.25	.050	.038	.044	..	.0404	.04064	.04067	.04117	..	..	.04025	.0850	.0444	.25
.20	.067	.056	.061	..	.0589	.05800	.05802	.05798	.05776	.05696	.05771	.0966	.0386	.20
.15	.092	.085	.088	..	.0880	.08620	.08621	.08538	..	..	.08568	.1171	.0309	.15
.10	.140	.140	.140	..	.1424	.14027	.14027	.13940	.14004	.13726	.14002	.1619	.0216	.10
.05	..	..	..	..	.2964	.29497	.29497	..	..	..	.29502	.3060	.0110	.05

No. XXXI.

ON THE COHESION OF FLUIDS,

BEING APPENDIX A. OF DR. YOUNG'S 'ELEMENTARY ILLUSTRATIONS OF THE CELESTIAL MECHANICS OF LAPLACE.'

PUBLISHED IN 1821.

1. THEOREM. If there be a series of equal particles, arranged at equal intervals in a right line, each attracting or repelling its immediate neighbour only with a constant force f; the force VM. acting on any obstacle M at one end of the whole line u, supposing the other to be fixed, will be equal to f.

The general principle of virtual velocities is $\Sigma m S\delta s = 0$ (see Illustrations, &c., p. 177), or, taking any one of the forces combined with each other, as the result of the rest, and in an opposite direction, $VM\delta u = \Sigma m S\delta s$: and in applying this principle, the variations may be taken in any manner capable of representing their relations to each other, without confining them to such as are likely to occur in the natural phenomena to be considered; and the motive force VM may always be found, if we can determine its equal $\frac{\Sigma m S\delta s}{\delta u}$. Now if the number of particles concerned be m, and their masses equal to unity, we shall have $\delta s = \frac{\delta u}{m}$, since we may suppose the particles to remain equally distributed throughout the line after the variation of their distances, and S being $= f$, we have $\Sigma m S\delta s = f\delta u$; consequently $VM = f$.

2. THEOREM. If an attractive or repulsive force extend to a given distance c among a series of m particles situated at equal distances in a right line, the mutual forces of any two particles being f, and their masses each unity, the tension acting on an obstacle at the end of the line u will be $\frac{mmcc}{uu} f$.

The number of particles in the line u being m, the number acting at

any one point will be $2m\frac{c}{u}$; and when the length u is varied, the variation of the distance of the remotest of these particles will be $\delta u\frac{c}{u}$, while that of the particles at a smaller distance will be proportionally smaller: and the mean variation of the distances of the particles within the respective spheres of action will be half the extreme variation. For each particle, therefore, the variation $\Sigma mS\delta s$ will be $\frac{1}{2}\ \delta u\frac{c}{u}\ 2c\ \frac{m}{u}f = \frac{cc}{uu}mf\,\delta u$, and for the whole line, consisting of m particles, $m^2\frac{cc}{uu}f\delta u$, which, divided by δu, gives $VM\ .\ \frac{mmcc}{uu}f$.

Corollary 1. Hence, if u be given, the tension will vary as the square of the number of particles or density m, and as the square of the extent of the sphere of action c, conjointly.

Corollary 2. If there be two forces, a cohesive force C, and a repulsive force R, holding each other in equilibrium, but extending to the different distances c and r, they will balance each other, in this hypothetical case, if $c^2\,C = r^2\,R$, that is, if the primitive forces of the single pairs of particles be inversely as the squares of the minute distances to which they extend.

Scholium. It is obvious that the length u is indifferent to the force, since m must vary as u, and $\frac{m}{u}$ must remain constant, when the density is given.

3. Theorem. If a fluid, composed of cohesive and repulsive particles, holding each other in equilibrium, be contained between two parallel surfaces, of unlimited extent, the equal and opposite forces, acting on either of the surfaces M, will be $\frac{\pi}{3}d^2\ c\ Mf$; d being the density, π the circumference of a circle divided by its diameter.

The number of particles in the space Mu being dMu, the number of those, which are within the limits of the sphere of action of each particle, will be $\frac{4}{3}\pi c^3 d$. Supposing now the distance of the particles to be varied by a slight change of the density, it is evident that the variation of the density will be in the duplicate proportion of that of the distances, since if $d = x^3$, $\mathrm{d}d = 3x^2\,\mathrm{d}x$; and the variation of the whole space Mu being $M\delta u$, that of the density $\delta d = -\delta u\frac{d}{u}$, and that of any

linear distance c will be $\delta c = -\frac{1}{8}\delta d\frac{c}{d} = \frac{1}{4}\delta u\frac{c}{u}$, which will be the variation of the distance of the particles, at the surface of the sphere of action, from its centre. But the mean distance of each elementary pyramid from its vertex, or of the whole sphere from the centre, is $\frac{3}{4}$ of the height or the radius, since the products of the elements of the content into the distance added together and divided by the content, or $\frac{Sx.x^2Dx}{Sx^2Dx} = \frac{3}{4}$. The mean variation of distance for the whole fluid is therefore $\frac{1}{4}\,c\,\frac{\delta u}{u}$; and this variation, multiplied by the number of particles within the sphere of action, becomes $\frac{d}{3}\pi c^4\frac{\delta u}{u}$; which being again multiplied by the number of centres Mud, and by the force f, and divided by δu, gives us $V = \frac{\pi}{3}d^2c^4Mf$, for the whole force acting on the surface M.

COROLLARY. In this case if the two forces C and R hold each other in equilibrium, we must have $c^4.C = r^4 R$, and C must be to R, for each pair of particles, as r^4 to c^4: each force still varying as the square of the density.

SCHOLIUM 1. The determination of the attractive or repulsive force of a sphere thus constituted may be illustrated and confirmed by a simpler mode of considering the joint action of the particles of each hemisphere which is easily shown to be half as great as if they were collected into one line. For it is obvious that each particle in any spherical surface must have its action on the central point reduced in the proportion that the radius bears to its distance from the plane dividing the hemispheres, consequently the whole force will be represented by the distance of the centre of gravity of the surface, multiplied into the mass, or the number of particles contained in it. Now the centre of gravity of a spherical surface is situated in the middle of its absciss or versed sine, since the increments of the surface are proportional to those of the versed sine. Hence it follows, that the joint force of all the particles in each surface is half what it would be, if they were all situated in the given direction: and the proportion being the same for all the concentric surfaces, it must also remain the same for the whole hemisphere. If we had only to consider the attractions of a series of particles, situated in a circular circumference, upon a central particle, it might be shown, in a similar manner, that they would be together equal to that of a number of particles represented by the chord, supposed to be placed at the middle of the arc.

SCHOLIUM 2. If any of the elastic fluids, with which we are acquainted, be considered as thus constituted, we must suppose the fourth power of

the distance r to vary inversely as the density d, since the force V is found to vary simply as the density, and $\frac{V}{d} = \frac{\pi}{3} dc^4 \, Mf$ is constant. It would have been more natural to expect, that if c were not constant, its cube c^3 would have varied inversely as the density, supposing the number of particles co-operating to be given. But in the Newtonian demonstration the elementary force f is also supposed to vary inversely as the distance, while the number of particles co-operating is invariable. In this case the number of particles in the space Mu are as dMu, and the elementary forces as $d^{-\frac{1}{3}}$, the variations of the distances, for a given value of δu, being as $d^{\frac{1}{3}}$, so that the products of these quantities remain constant, and the effective force is as the number of particles concerned, or simply as d.

4. LEMMA. If the height of a cone be a, the radius of the base b, and the oblique side c, the mean distance of the base from the vertex will be $\frac{2}{3} \cdot \frac{c^3 - a^3}{b^2}$.

For, if the fluxion of the radius of the base be $\dot{x}$, the product of the elementary ring $2\pi x \Delta x$, into its distance $\sqrt{(a^2+x^2)}$, will be $2\pi x \Delta x \sqrt{(a^2+x^2)}$; and since $\dot{\left\{(a^2+x^2)^{\frac{3}{2}}\right\}} = \frac{3}{2} \times 2x\dot{x} \sqrt{(a^2+x^2)}$, we have $\int 2\pi x \sqrt{(a^2+x^2)}\, \dot{x} = \frac{2\pi}{3}(a^2+x^2)^{\frac{3}{2}}$, which becomes initially $\frac{2\pi}{3} a^3$, and when $x = b$, $\frac{2\pi}{3} c^3$, and the difference, divided by πb^2, the area of the base, that is, $\frac{2}{3} \frac{c^3 - a^3}{b^2}$, or $\frac{2}{3} \frac{c^3 - a^3}{c^2 - a^2}$, will be the mean distance of the base from the vertex.

COROLLARY. For a solid cone, the mean distance becomes $\frac{3}{4}$ of that of the base, as in the case of the sphere: and the expression becomes, in this case, $\frac{1}{2} \frac{c^3 - a^3}{c^2 - a^2}$.

5. THEOREM. The deficiency of the mutual actions of the superficial particles of a fluid, of limited extent, deducts from the tension $\frac{1}{4}$ of the whole force of a stratum equal in thickness to the radius of the sphere of equal action.

For the interior parts of the fluid, the actions of all the particles will be the same as in a fluid of unlimited extent, that is, $\frac{\pi}{3} c^4 \, Mf$, calling the density unity, since its finite variations do not enter into the present

question. But for the particles within the distance c of the surface, the forces will be able to act on such a number of other particles only, as are contained in a segment of the sphere, of which the versed sine is $c+x$, the distance from the surface being x, which are not only fewer than in the whole sphere, but are also at a smaller mean distance from the centre.

Each of these segments may be divided into two portions; that which is contained between the centre and the spherical circumference, and the cone, which lies between the centre and the plane surface: the variation of the mean distance of the former will be the same as for the whole sphere; but for the cone, instead of the variation belonging to that of the corresponding portion of the sphere, which will be expressed by the product of its contents into $\frac{3}{4}$ of the variation of the radius, we shall have the content of the cone into the variation of its mean distance, or $\frac{\pi}{3}(c^2-x^2)x$ into $\frac{1}{2}\frac{c^3-x^3}{c^2-x^2}\cdot\frac{\delta u}{u}$ that is, $\frac{\pi}{6}(c^3-x^3)x\frac{\delta u}{3u}$, instead of $2\pi c(c-x)\frac{c}{3}$ into $\frac{3}{4}c\frac{\delta u}{3u}$, or $\frac{\pi}{2}(c^4-c^3x)\frac{\delta u}{3u}$, the difference being $\frac{\pi}{6}(3c^4-4c^3x+x^4)\frac{\delta u}{3u}$, for each particle at the distance x from the surface; and in order to find the total difference for the whole stratum, we must multiply this by the fluxion of x, and find the fluent, which will be $\frac{\pi}{6}(3c^4x-2c^3x^2+\frac{1}{5}x^5)\frac{\delta u}{3u}$ or, when $x=c$, $\frac{\pi}{6}\cdot\frac{6}{5}c^5\frac{\delta u}{3u}=\frac{\pi}{15}c^5\frac{\delta u}{u}$, and for the length u, $\frac{\pi}{15}c^5\delta u$, while the force of the whole stratum of the thickness c, would have been $\frac{\pi}{3}c^4c$, substituting c for M in article 4, and the deficiency is to the whole force as $\frac{1}{15}$ to $\frac{1}{5}$ or as 1 to 5.)

Corollary. If the cohesive force C and the repulsive R be in equilibrium for the whole fluid considered as incomparably greater in thickness than c or r, the difference of the forces with regard to the superficial stratum on each side only, will be $\frac{1}{5}\cdot\frac{\pi}{3}(c^5C-r^5R)$: now it has been shown that $c^4C=r^4R$, consequently $c^5C-r^5R+c^4C(c-r)$, and the joint deficiency in the cohesive force will be $\frac{1}{5}\cdot\frac{\pi}{3}c^5C\left(1-\frac{r}{c}\right)$.

Corollary 2. The deficiency being positive when c is greater than r, it follows that if the superficial cohesion prevail in a fluid so constituted, it must be because r is greater than c and the defect is greatest with regard to the repulsive force. In such cases the fluid must be

slightly condensed in its interior parts, so as to produce a resistance equivalent to the excess of cohesion of the surface.

COROLLARY 3. These conclusions are applicable, with slight modifications only, to the case of a repulsion like that of elastic fluids, as assumed by Newton. For we have only to take r equal to the radius of the actual mean sphere of action for the fluid in any given state of compression, and the superficial deficiency of the force will be very nearly as determined by this proposition, the distance r becoming in this case somewhat smaller than the whole extent of the sphere of action. The utmost possible cohesive force would be obtained from the supposition that c is incomparably smaller than r, and this force would be $\frac{1}{5}\frac{\pi}{3}r^5R$, or $\frac{1}{5}$ of the repulsive force of a stratum of the interior part of the fluid of the thickness r; but in every case that can actually occur, the superficial force must probably be much less than this.

SCHOLIUM. On the whole we are fully justified in concluding that, since the phenomena of capillary action necessarily lead us to infer the existence of a superficial tension, and since without this supposition, we should be obliged to admit the possibility of a perpetual source of motion, from an unequal hydrostatic pressure, upon any floating body not homogeneous; the existence of such a cohesive tension proves that the mean sphere of action of the repulsive force is more extended than that of the cohesive: a conclusion, which, though contrary to the tendency of some other modes of viewing the subject, shows the absolute insufficiency of all theories built upon the examination of one kind of corpuscular force alone. It must also be recollected that, as far as our experiments enable us to observe, the repulsive force of solids does actually extend further than the cohesive, though, with respect to its mean intensity, we have no direct method of ascertaining the comparative extent of the spheres of action of the two forces.*

* Dr. Young refers, in the Preface of the work, from which the Article in the text is taken, to the investigations which it contains as forming an important step in this difficult theory, by aiding our conception of the existence of superficial cohesion, consistently with the known laws of the pressure of fluids.—*Note by the Editor.*

No. XXII.

HYDRAULIC INVESTIGATIONS.

From the Philosophical Transactions for 1808.

Read May 5, 1808.

I.—*Of the Friction and Discharge of Fluids running in Pipes, and of the Velocity of Rivers.*

Having lately fixed on the discussion of the nature of inflammation for the subject of an academical exercise,* I found it necessary to examine attentively the mechanical principles of the circulation of the blood, and to investigate minutely and comprehensively the motion of fluids in pipes, as affected by friction, the resistance occasioned by flexure, the laws of the propagation of an impulse through the fluid contained in an elastic tube, the magnitude of a pulsation in different parts of a conical vessel, and the effect of a contraction advancing progressively through the length of a given canal. The physiological application of the results of these inquiries I shall have the honour of laying before the Royal Society at a future time; but I have thought it advisable to communicate, in a separate paper, such conclusions, as may be interesting to some persons, who do not concern themselves with disquisitions of a physiological nature; and I imagine it may be as agreeable to the Society that they should be submitted at present to their consideration, as that they should be withheld until the time appointed for the delivery of the Croonian Lecture.

It has been observed by the late Professor Robison, that the comparison of the Chevalier Dubuat's calculations with his experiments is in all respects extremely satisfactory; that it exhibits a beautiful specimen of the means of expressing the

* On taking the degree of M.D. at Cambridge. This exercise has not been preserved.—*Note by the Editor.*

general result of an extensive series of observations in an analytical formula, and that it does honour to the penetration, skill, and address of Mr. Dubuat, and of Mr. De St. Honoré, who assisted him in the construction of his expressions.* I am by no means disposed to dissent from this encomium; and I agree with Professor Robison, and with all other late authors on hydraulics, in applauding the unusually accurate coincidence between these theorems and the experiments from which they were deduced. But I have already taken the liberty of remarking, in my lecture on the history of hydraulics,† that the form of these expressions is by no means so convenient for practice as it might have been rendered; and they are also liable to still greater objections in particular cases, since, when the pipe is either extremely narrow, or extremely long, they become completely erroneous: for notwithstanding Mr. Dubuat seems to be of opinion, that a canal may have a finite inclination, and yet the water contained in it may remain perfectly at rest, and that no force can be sufficient to make water flow in any finite quantity through a tube less than one twenty-fifth of an inch in diameter, it can scarcely require an argument to show that he is mistaken in both these respects. It was therefore necessary for my purpose to substitute, for the formulæ of Mr. Dubuat, others of a totally different nature; and I could follow Dubuat in nothing but in his general mode of considering a part of the pressure, or of the height of a given reservoir, as employed in overcoming the friction of the pipe through which the water flows out of it; a principle, which, if not of his original invention, was certainly first reduced by him into a practical form. By comparing the experiments, which he has collected, with some of Gerstner, and some of my own, I have ultimately discovered a formula, which appears to agree fully as well as Dubuat's, with the experiments from which his rules were deduced, which accords better with Gerstner's experiments, which extends to all the extreme cases with equal accuracy, which seems to represent more simply the actual operation of the forces concerned, and which is directed in its

* See Sir David Brewster's edition of Dr. Robison's works, vol. ii. p. 450, 'Theory of Rivers.'—*Note by the Editor.* † Lecture xxx., p. 394.

application to practice, without the necessity of any successive approximations.*

I began by examining the velocities of the water, discharged, through pipes of a given diameter, with different degrees of pressure; and I found that the friction could not be represented by any single power of the velocity, although it frequently approached to the proportion of that power, of which the exponent is 1.8; but that it appeared to consist of two parts, the one varying simply as the velocity, the other as its square. The proportion of these parts to each other must however be considered as different, in pipes of different diameters, the first part being less perceptible in very large pipes, or in rivers, but becoming greater than the second in very minute tubes, while the second also becomes greater, for each given portion of the internal surface of the pipe, as the diameter is diminished.

If we express in the first place, all the measures in French inches, calling the height employed in overcoming the friction f, the velocity in a second v, the diameter of the pipe d, and its length l, we may make $f = a\frac{l}{d}v^2 + 2c\frac{l}{d}v$; for it is obvious that the friction must be directly as the length of the pipe; and since the pressure is proportional to the area of the

* In the article Hydraulics, which appeared in the Supplement of the Encyclopædia Britannica, Dr. Young has not only recapitulated the principal results of this Memoir, but has also noticed the earlier attempts made by some other authors to improve this department of science, and more especially those of Prony in his 'Recherches Physico-Mathématiques sur la théorie des eaux courantes,' which were published in 1804. "Ingenious and important," says he, "as the Chevalier Dubuat's theory of the friction of fluids is admitted to have been, it cannot be denied that it is extremely deficient both in the distinct elucidation of the physical grounds of the phenomena and with respect to the neatness and simplicity of the methods of calculation." M. Gerard, according to M. de Prony, was the first who entertained the fortunate idea of applying the theory of M. Coulomb to the resistance of water flowing in pipes and canals: and in his two Memoirs on the 'Theory of Running Waters,' he proposes a formula dependent on the sum of the velocity and its square, for the expression of the friction; and deducing a constant coefficient from twelve experiments of Chezy and Dubuat, he obtains a formula equally correct with that of Dubuat and far more simple.

"M. Prony has obtained an expression founded on the same theoretical principles that M. Gerard had adopted, but much more perfect and accurate and agreeing very sufficiently with all those of Dubuat's experiments to which he has applied it. But M. de Prony's formula, as well as Dubuat's, fails altogether when we attempt to employ it for the computation of the discharge through very slender tubes; and though his work was printed a year or two earlier than Dr. Young's investigations, it would have been impossible for him to make any use of it in his physiological inquiries, even if it had not been wholly unknown to him, since the resistances he had to compute were principally such as occurred in tubes considerably less than a thousandth of an inch in diameter."—*Note by the Editor.*

section, and the surface producing the friction to its circumference or diameter, the relative magnitude of the friction must also be inversely as the diameter, or nearly so, as Dubuat has justly observed. We shall then find that a must be $.0000001\left(430 + \frac{75}{d} - \frac{1440}{d+12} - \frac{180}{d+\frac{1}{3}}\right)$, and $c = .0000001\left(\frac{900dd}{dd+1000} + \frac{1}{\sqrt{d}}\left(1050 + \frac{12}{d} + \frac{.9}{dd}\right)\right)$.* Hence it is easy to calculate the velocity for any given pipe or river, and with any given head of water. For the height required for producing the velocity, independently of friction, is, according to Dubuat, $\frac{v^2}{478}$, or rather as it appears from almost all the experiments which I have compared, $\frac{v^2}{550}$; and the whole height h is therefore equal to $f + \frac{v^2}{550}$, or $h = \left(\frac{al}{d} + \frac{1}{550}\right)v^2 + \frac{2cl}{d}v$; and making $b = \frac{1}{al:d+.00182}$, and $e = \frac{bcl}{d}$, $v^2 + 2ev = bh$, whence $v = \sqrt{(bh + e^2)} - e$. In order to adapt this formula to the case of rivers, we must make l infinite; then b becomes $\frac{d}{al}$, and $bh = \frac{d}{a} \cdot \frac{h}{l} = \frac{ds}{a}$, s being the sine of the inclination, and d four times the hydraulic mean depth; and since e is here $= \frac{c}{a}$, $v = \frac{\sqrt{(ads + cc)} - c}{a}$, and in most rivers, v becomes nearly $\sqrt{(20000\,ds)}$.

In order to show the agreement of these formulæ with the result of observation, I have extracted, as indiscriminately and impartially as possible, forty of the experiments made and collected by Dubuat; I have added to these some of Gerstner's, with a few of my own; and I have compared the results of these experiments with Dubuat's calculations, and with my own formulæ, in separate columns. There are six of Dubuat's experiments which he has rejected as irregular, apparently without any very sufficient reason, since he has accidentally mentioned that some of them were made with great care: I have therefore calculated the velocities for these experiments in both ways, and compared the results in the separate table on p. 495.

* It should always be kept in mind, that these and the subsequent formulæ are almost entirely empirical.—*Note by the Editor.*

Tabular Comparison of Hydraulic Experiments.

Observer.	*d.*	$\frac{1}{s}$	Superf. Veloc.	*v*	Dub.	Log. ratio.	Y.	Log. ratio.	*a* .17×	*c* .17×	$\sqrt{}$ (20000*ds.*)
Dubuat	262.5	35723	15.96	12.56?	10.53	.0776	11.10	.0537	424	952	11.1
	258.5	6413	31.77	26.63?	28.76	.0334	28.02	.0221	424	952	28.3
	92.4	21827	.9.61	7.01?	8.38	.0775	8.14	.0649	415	914	9.3
	75.6	27648	7.27	5.07?	6.55	.1112	6.27	.0923	413	887	7.5
	17.6	9288	..	5.70	5.86	.0120	5.97	.0291	376	465	6.1
	16.4	432	..	32.52	31.61	.0124	30.67	.0255	374	451	27.6
	11.7	1412	..	14.17	13.59	.0182	14.05	.0037	360	416	12.2
	9.9	427	..	22.37	24.37	.0372	24.41	.0379	355	414	21.7
	5.8	212	..	27.51	27.19	.0051	27.34	.0027	332	466	23.5

Observers.	*d.*	*l.*	*h.*	*v.*	Dub.	Log. ratio.	Y.	Log. ratio.	*a* .17×	.17×
Couplet	18	43200	145.08	39.16	40.51	.0148	38.49	.0075	376	469
	5	84240	25.00	5.32	5.29	.0024	5.40	.0065	326	492
			16.75	4.13	4.23	.0103	4.21	.0083		
			5.58	2.01	2.25	.0490	2.01	.0000		
Bossut	2.01	2160	24	24.73	24.08	.0115	24 76	.0006	287	747
			12	16.38	16.10	.0075	16.86	.0125		
		1080	24	35.77	35.10	.0082	35.05	.0089		
		360	24	58.90	58.80	.0007	56.85	.0154		
	1.33	2160	12	12.56	12.75	.0065	13.28	.0242	270	919
		1080	24	28 08	28.21	.0020	28.84	.0116		
		360	24	48.53	49.52	.0088	48.66	.0015		
	1.	600	12	22.28	21.98	.0055	22.83	.0106	259	1063
			4	12.22	11.76	.0167	11.92	.0108		
Dubuat		737	23.7	28.67	29.41	.0111	30.11	.0213		
			12.2	19.99	19.95	.0009	20.67	.0145		
			4.2	10.56	10.66	.0041	10.90	.0137		
		117	36	84.95	85.52	.0029	83.12	.0069		
			18	58.31	58.47	.0014	58.41	.0012		
	.24167	36.25	53.25	85.77	85.20	.0029	85.71	.0003	309	2268
			41.25	73.81	73.90	.0005	74.67	.0050		
			20.17	51.96	50.14	.0155	50.87	.0093		
			5.00	23.40	23.19	.0039	23.09	.0058		
			.83	7.58	8.22	.0420	7.22	.0212		
	.1667	36.25	51.25	64.37	64.95	.0031	64.08	.0021	402	2827
			38.75	54.19	55.32	.0090	54.93	.0055		
			15.29	33.38	33.17	.0028	32.67	.0094		
			2.04	10 62	10.49	.0053	9.24	.0604		
	125	34.17	42.17	45.47	46.21	.0070	45.88	.0039	518	3405
			35.33	41.61	41.71	.0010	41.55	.0006		
			14.58	26.20	25.52	.0114	24.94	.0214		
			2.08	7.32	8.35	.0572	6.98	.0206		
					Mean = L.	.0178 1.042	Mean = L.	.0169 1.040		

Observers.	*d.*	*l.*	*h.*	*v.*	Dub.	Log. rat.	Y.	Log. rat.	*a.*	*c.*
Gerstner at 55.5°F.	.2	63	10.7	24.2	23.9	.006	24.1	.002	349	2533
			7.7	21.0	19.9	.023	19.1	.042		
			4.7	15.8	14.9	.026	13.9	.056		
			1.7	7.5	8.2	.039	6.9	.036		
			.7	2.5	5.0	.301	3.4	.133		
	.133	33	10.7	27.1	23.4	.064	22.5	.081	488	3259
			7.7	23.2	19.4	.077	18.5	.098		
			4.7	15.4	14.6	.024	13.5	.058		
			1.7	5.6	8.1	.160	6.7	.078		
			.7	2.3	4.6	.301	3.4	.169		
	.0674	33	10.7	10.0	8.9	.051	10.1	.004	975	5700
			7.7	7.2	7.4	.012	8.2	.057		
			4.7	4.5	5.6	.095	5.6	.095		
			1.7	1.5	3.1	.316	2.5	.222		
			.7	.5	1.8	.444	1.1	.342		
					Mean .129		Mean .098			
					=L. 1.346		=L. 1.254			
Y. at 60°.	$\frac{1}{42}$	8.50	32.4	14.40	0	∞	13.36	.032	2956	13882
	$\frac{1}{183}$	3.42	30.0	.53			.52	.008	13404	452100
		1.17	5.8	.27			.30	.046		
							Mean .029			
							=L. 1.068			
Dubuat .	2	255.25	36.35	86.31	84.2	.011	79.7	.035	287	747
	1	24	36.25	122.59	117.8	.018	120.8	.007	259	1063
			27	106.45	101.1	.022	104.1	.010		
			18	84.85	82.2	.013	84.8	.000		
			9	59.25	57.5	.013	59.7	.004		
		4	27.08	118.67	111.5	.027	118.5	.000		
					Mean .017		Mean .009			
					=L. 1.041		=L. 1.022			

It appears from this comparison, that in the forty experiments extracted from the collection, which served as a basis for Dubuat's calculations, the mean error of his formula is $\frac{1}{24}$ of the whole velocity, and that of mine $\frac{1}{25}$ only; but if we omit the four experiments, in which the superficial velocity only of a river was observed, and in which I have calculated the mean velocity by Dubuat's rules, the mean error of the remaining 36 is $\frac{1}{35}$, according to my mode of calculation, and $\frac{1}{37}$ according to Mr. Dubuat's; so that on the whole, the accuracy of the two formulæ may be considered as precisely equal with respect to these experiments. In the six experiments which Dubuat has wholly rejected, the mean error of his formula is about $\frac{1}{24}$,

and that of mine $\frac{1}{45}$. In fifteen of Gerstner's experiments the mean error of Dubuat's rule is one third, that of mine one fourth; and in the three experiments which I made with very fine tubes, the error of my own rules is one fifteenth of the whole, while in such cases Dubuat's formulæ completely fail.

I have determined the mean error by adding together the logarithmic ratios of all the results, and dividing the sum by the number of experiments. It would be useless to seek for a much greater degree of accuracy, unless it were probable that the errors of the experiments themselves were less than those of the calculations; but if a sufficient number of extremely accurate and frequently repeated experiments could be obtained, it would be very possible to adapt my formula still more correctly to their results.

In order to facilitate the computation, I have made a table of the coefficients a and c for the different values of d, all the measures being still expressed in French inches.

Table of Coefficients for French Inches.

d	a .17×	c .17×	d	a .17×	c .17×	d	a .17×	c .17×	d	a .17×	c .17×
∞	430	900	40	400	719	4	319	540	.4	257	1717
500	427	943	30	393	618	3	305	617	$\frac{1}{3}$	268	1895
400	426	945	25	387	560	2.5	296	687	.3	279	2008
300	423	950	20	380	492	2	288	751	$\frac{1}{4}$	303	2225
200	421	951	15	370	427	1.5	275	866	.2	349	2532
100	416	923	10	354	414	1	259	1063	$\frac{1}{6}$	402	2827
90	415	911	9	350	421	.9	255	1123	.15	440	3026
80	413	896	8	345	433	.8	252	1193	$\frac{1}{7}$	458	3116
70	410	872	7	340	440	.7	249	1278	$\frac{1}{8}$	518	3405
60	408	840	6	335	462	.6	248	1384	$\frac{1}{9}$	589	3693
50	406	792	5	325	512	.5	249	1524	.1	646	3985

For example, in the last experiment, where d is 1, l 4, and h 27.1, we have $a = .0000259$, $b = \frac{1}{al : d + .00182} = 516$, $c = .0001063$, $e = bcl : d = .22$, and $v = \sqrt{(bh + e^2)} - e = 118.46$, which agrees with the experiment within $\frac{1}{300}$ of the whole. I had at first employed for a the formula $\frac{430}{1 + 12 : d} + \frac{57}{d} + \frac{1}{6dd}$,

but I found that the value thus determined, became too great when d was about 20, and too small in some other cases. Coulomb's experiments on the friction of fluids, made by means of the torsion of wires, give about .00014 for the value of c, which agrees as nearly with this table, as any constant number could be expected to do. I have however reason to think, from some experiments communicated to me by Mr. Robertson Buchanan, that the value of a, for pipes above half an inch in diameter, is somewhat too small; my mode of calculation, as well as Dubuat's, giving too great a velocity in such cases.*

If any person should be desirous of making use of Dubuat's formula, it would still be a great convenience to begin by determining v according to this method; then, taking $b = \frac{l}{h - v^2 : 478}$, or rather, as Langsdorf makes it, $b = \frac{l}{h - v^2 : 482}$, to proceed in calculating v by the formula $v = 148.5 (\sqrt{d} - .2) \cdot \left(\frac{1}{\sqrt{b} - \text{H.L.}\sqrt{(b + 1.6)}} - .001\right)$, since this determination of b will, in general, be far more accurate than the

* "It must be confessed," says Dr. Young in the article Hydraulics referred to before, "that notwithstanding the convenience of this theory for calculation, with the assistance of the tables of coefficients, their determination from the diameters of the pipes is somewhat too complicated either for elegance or probability, if considered as representing the law of nature. The formula of Prony, though it fails for small pipes, has the advantage of great simplicity, and even of superior accuracy, within certain limits of the magnitude of the pipes, although it seems to be indebted for this accuracy to accidental causes only."

"If we take the equation $v = \frac{\sqrt{(ads + c^2)} - c}{a}$ given in page 494, and assume for a the constant mean value .0000377, and for c, .00003466, we shall have $v = \sqrt{(26520ds + .845)} - .919$, which is equivalent to Prony's formula reduced to French inches. For pipes, M. de Prony merely substitutes $\frac{h}{l}$ for s, neglecting entirely the height $\frac{v^2}{550}$ due to the velocity. He also gives a still simpler approximation for common purposes, $v = \sqrt{\left(26513\frac{dh}{l}\right)}$ or $\sqrt{\left(26513ds\right)}$ which differs very little from the rate given by Dr. Young in his Lectures, which is "that the velocity is a mean proportional between the hydraulic mean depth and the fall in 2800 yards, for this, in French measures, would be $\sqrt{(27000ds)}$. It is obvious, however, that, in many cases, these formulæ must require considerable modification, since, when the velocity is great, the height due to it may become considerable, and since the friction in small pipes is certainly increased beyond its mean value: nor can these opposite causes of error be expected always to compensate each other even in pipes of moderate dimensions."—*Note by the Editor.*

simple expression $b = \frac{l + 45d}{h}$, and the continued repetition of the calculation, with approximate values of v, may thus be avoided. Sometimes, indeed, the values of v found by this repetition, will constitute a diverging instead of a converging series, and in such cases, we can only employ a conjectural value of v, intermediate between the two preceding ones.

Having sufficiently examined the accuracy of my formula, I shall now reduce it into English inches, and shall add a second table of the coefficients, for assisting the calculation. In this case, a becomes $.0000001\left(413 + \frac{75}{d} - \frac{1440}{d + 12.8} - \frac{180}{d + .355}\right)$, $c = .0000001\left(\frac{900dd}{dd + 1136} + \frac{1}{\sqrt{d}}\left(1085 + \frac{1321}{d} + \frac{1.0563}{dd}\right)\right)$, and $b = \frac{1}{al : d + .00171}$, e being $\frac{bcl}{d}$, and $v = \sqrt{(bh + e^2)} - e$, or $= \sqrt{\left(\frac{ds}{a} + \frac{cc}{aa}\right)} - \frac{c}{a}$, as before; and in either case the superficial velocity of a river may be found, very nearly, by adding to the mean velocity v its square root, and the velocity at the bottom by subtracting it.

Table of Coefficients for English Inches.

d	$\frac{a}{.17}\times$	$\frac{c}{.17}\times$	d	$\frac{a}{.17}\times$	$\frac{c}{.17}\times$	d	$\frac{a}{.17}\times$	$\frac{c}{.17}\times$	d	$\frac{a}{.17}\times$	$\frac{c}{.17}\times$
∞	413	900	40	383	698	4	306	556	.4	254	1779
500	410	944	30	377	597	3	292	635	$\frac{1}{3}$	268	1963
400	409	948	25	371	526	2.5	284	694	.3	280	2082
300	406	951	20	364	482	2	277	774	$\frac{1}{4}$	305	2307
200	404	951	15	354	430	1.5	266	894	.2	354	2631
100	399	918	10	339	413	1	251	1099	$\frac{1}{6}$	409	2943
90	398	903	9	336	421	.9	248	1161	.15	447	3150
80	396	885	8	331	433	.8	245	1234	$\frac{1}{7}$	466	3251
70	393	860	7	327	449	.7	243	1322	$\frac{1}{8}$	528	3558
60	391	825	6	322	471	.6	243	1433	$\frac{1}{9}$	599	3866
50	389	772	5	312	507	.5	245	1578	.1	657	4183

II.—*Of the Resistance occasioned by Flexure in Pipes or Rivers.*

Mr. Dubuat has made some experiments on the effect of the flexure of a pipe in retarding the motion of the water flowing through it; but they do not appear to be by any means sufficient to authorise the conclusions which he has drawn from them. He directs the squares of the sines of the angles of flexure to be collected into one sum, which, being multiplied by a certain constant coefficient, and by the square of the velocity, is to show the height required for overcoming the resistance. It is, however, easy to see that such a rule must be fundamentally erroneous, and its coincidence with some experiments merely accidental, since the results afforded by it must vary according to the method of stating the problem, which is entirely arbitrary. Thus it depended only on Mr. Dubuat to consider a pipe bent to an angle of 144° as consisting of a single flexure, as composed of two flexures of 72° each, or of a much greater number of smaller flexures, although the result of the experiment would only agree with the arbitrary division into two parts, which he has adopted. This difficulty is attached to every mode of computing the effect either from the squares of the sines or from the sines themselves; and the only way of avoiding it is to attend merely to the angles of flexure as expressed in degrees. It is natural to suppose that the effect of the curvature must increase, as the curvature itself increases, and that the retardation must be inversely proportional to the radius of curvature, or very nearly so; and this supposition is sufficiently confirmed, by the experiments which Mr. Dubuat has employed in support of a theory so different. It might be expected that an equal curvature would create a greater resistance in a larger pipe than in a smaller, since the inequality in the motions of the different parts of the fluid is greater; but this circumstance does not seem to have influenced the results of the experiments made with pipes of an inch and of two inches diameter: there must also be some deviation from the general law, in cases of very small pipes

having a great curvature, but this deviation cannot be determined without further experiments. Of the 25 which Dubuat has made, he has rejected 10 as irregular, because they do not agree with his theory: indeed 4 of them, which were made with a much shorter pipe than the rest, differ so manifestly from them that they cannot be reconciled: but 5 others agree sufficiently, as well as all the rest, with the theory which I have here proposed, supposing the resistance to be as the angular flexure, and to increase besides almost in the same proportion as the radius of curvature diminishes, but more nearly as that power of the radius of which the index is $\frac{7}{8}$. Thus if p be the number of degrees subtended at the centre of flexure, and q the radius of curvature of the axis of the pipe in French inches, we shall have $r = \frac{pv^2}{200000q}$ nearly, or, more accurately, $r = \frac{.0000045pv^2q^{\frac{1}{8}}}{q}$. These calculations are compared with the whole of Dubuat's experiments in the following table.

Table of Experiments on the Resistance occasioned by Flexure.

p	q	v^2	r	B.	Y. 1	Y. 2	p	q	v^2	r	B.	Y.1	Y.2
288	3.22	15030	4.75		6.71	6.98	288	3.22	3415	1.50	1.57	1.52	1.58
		11330	3.50		5.06	5.26	144			.75	.78	.76	.79
		7199	2.33		3.21	3.34	72			.37	.39	.38	.39
		3510	1.08		1.56	1.62	196.5	6.12		.75	.78	.55	.62
216		7216	2.49	2.49	2.42	2.52	112.5	.53		1.50		3.63	3.00
144			1.50	1.66	1.61	1.67	720	3.22	5125	5.90	5.90	5.72	5.95
72			.75	.83	.80	.83	288		3458	1.64	1.59	1.54	1.60
196.5	6.12		1.50	1.66	1.16	1.31			860	.41	.40	.38	.40
147.4			1.12	1.24	.87	.98			821	.39	.38	.37	.38
98.3			.75	.83	.58	.65	288	4.10	3448	1.33		1.21	1.30
49.1			.37	.41	.29	.33			7449	2.90		2.59	2.78
112.5	.53		6.00		7.68	6.36	294.8	9.9					
99			5.90		6.74	5.60	360	4.1		8.64		8.08	8.62
288	3.22	3415	1.50	1.57	1.52	1.58	112.5	1.1					

In the last three experiments, the diameter of the pipe was two inches. The radius of curvature is not ascertained within the tenth of an inch, as Dubuat has not mentioned the thickness of the pipes. The mean error of his formula in fifteen experiments, and of mine in twenty, is $\frac{1}{25}$ of the whole.

III.—*Of the Propagation of an Impulse through an elastic Tube.*

The same reasoning that is employed for determining the velocity of an impulse, transmitted through an elastic solid or fluid body, is also applicable to the case of an incompressible fluid contained in an elastic pipe; the magnitude of the modulus being properly determined according to the excess of pressure which any additional tension of the pipe is capable of producing; its height being such as to produce a tension, which is to any small increase of tension produced by the approach of two sections of the fluid in the pipe, as their distance to its decrement: for in this case the forces concerned are precisely similar to those which are employed in the transmission of an impulse through a column of air enclosed in a tube, or through an elastic solid. If the nature of the pipe be such, that its elastic force varies as the excess of its circumference or diameter above the natural extent, which is nearly the usual constitution of elastic bodies, it may be shown that there is a certain finite height which will cause an infinite extension, and that the height of the modulus of elasticity, for each point, is equal to half its height above the base of this imaginary column; which may therefore be called with propriety the modular column of the pipe: consequently the velocity of an impulse will be at every point equal to half of that which is due to the height of the point above the base; and the velocity of an impulse ascending through the pipe being every where half as great as that of a body falling through the corresponding point in the modular column, the whole time of ascent will be precisely twice as great as that of the descent of the falling body; and in the same manner, if the pipe be inclined, the motion of the impulse may be compared with that of a body descending or ascending freely along an inclined plane.

These propositions may be thus demonstrated: let a be the diameter of the pipe in its most natural state, and let this diameter be increased to b by the pressure of the column c, the tube being so constituted that the tension may vary as the

force. Then the relative force of the column c is represented by bc, since its efficacy increases, according to the law of hydrostatics, in the ratio of the diameter of the tube; and this force must be equal, in the state of equilibrium, to the tension arising from the change from a to b, that is, to $b-a$; consequently the height c varies as $\frac{b-a}{b}$; and if the tube be enlarged to any diameter x, the corresponding pressure required to distend it will be expressed by a height of the column equal to $\left(1-\frac{a}{x}\right)\cdot\frac{bc}{b-a}$, since $\frac{b-a}{b} : c :: \frac{x-a}{x} : \left(1-\frac{a}{x}\right)\frac{bc}{b-a}$. Now if the diameter be enlarged in such a degree, that the length of a certain portion of its contents may be contracted in the ratio $1 : 1-r$, r being very small, then the enlargement will be in the ratio $1 : 1+\frac{r}{2}$, that is, x' will be $\frac{rx}{2}$; but the increment of the force, or of the height, is $\frac{ax'}{xx}\cdot\frac{bc}{b-a}$, which will become $\frac{ar}{2x}\cdot\frac{bc}{b-a}$. Now in a tube filled with an elastic fluid, the height being h, the force in similar circumstances would be rh, and if we make $h=\frac{a}{2x}\cdot\frac{bc}{b-a}$, the velocity of the propagation of an impulse will be the same in both cases, and will be equal to the velocity of a body which has fallen through the height $\frac{1}{2}h$. Supposing x infinite, the height capable of producing the necessary pressure becomes $\frac{bc}{b-a}$, which may be called g, and for every other value of x this height is $\left(1-\frac{a}{x}\right)g$, or $g-\frac{ag}{x}$, or $g-2h$, since h becomes $\frac{ag}{2x}$, so that h is always equal to half the difference between g and the actual height of the column above the given point, or to half the height of the point above the base of the column.

If two values of x, with their corresponding heights, are given as b and x, corresponding to c and d, and it is required to find a; we have $\frac{b-a}{b} : c :: \frac{x-a}{x} : d$, $dbx-dax=cbx-cba$, and $a=\frac{dbx-cbx}{dx-cb}$, or $\frac{b}{a}=\frac{dx-cb}{dx-cx}$. Thus if the height equivalent to the tension vary in the ratio of any power m of the

diameter, so that, n being a small quantity, $x = b\ (1 + n)$ and $d = c\ (1 + mn)$, $\frac{b}{a} = \frac{bc\left((1+n).\ (1+mn) - 1\right)}{bc\left((1+n).\ (1+mn) - (1+n)\right)} = \frac{mn+n}{mn}$, since the square of n is evanescent, and $\frac{b}{a} = \frac{m+1}{m}$. For example, if $m = 4$, $\frac{b}{a} = \frac{5}{4}$, and if $m = 2$, $b : a :: 3 : 2$.

IV.—*Of the Magnitude of a diverging Pulsation at different Points.*

The demonstrations of Euler, Lagrange, and Bernouilli, respecting the propagation of sound, have determined that the velocity of the actual motion of the individual particles of an elastic fluid, when an impulse is transmitted through a conical pipe, or diverges spherically from a centre, varies in the simple inverse ratio of the distance from the vertex or centre, or in the inverse subduplicate ratio of the number of particles affected, as might naturally be inferred from the general law of the preservation of the ascending force or impetus, in all cases of the communication of motion between elastic bodies, or the particles of fluids of any kind. There is also another way of considering the subject, by which a similar conclusion may be formed respecting waves diverging from or converging to a centre. Suppose a straight wave to be reflected backwards and forwards in succession, by two vertical surfaces, perpendicular to the direction of its motion; it is evident that in this and every other case of such reflections, the pressure against the opposite surfaces must be equal, otherwise the centre of inertia of the whole system of bodies concerned would be displaced by their mutual actions, which is contrary to the general laws of the properties of the centre of inertia. Now if, instead of one of the surfaces, we substitute two others, converging in a very acute angle, the wave will be elevated higher and higher as it approaches the angle: and if its height be supposed to be every where in the inverse subduplicate ratio of the distance of the converging surfaces, the magnitude of the pressure reduced to the direction of the motion, will be precisely equal to that of the pressure on the single opposite surface, which will not happen if the elevation vary inversely in the simple ratio of the

distance, or in that of any other power than its square root. This mode of considering the subject affords us therefore an additional reason for asserting, that in all transmissions of impulses through elastic bodies, or through gravitating fluids, the intensity of the impulse varies inversely in the subduplicate ratio of the extent of the parts affected at the same time; and the same reasoning may without doubt be applied to the case of an elastic tube.

There is however a very singular exception, in the case of waves crossing each other, to the general law of the preservation of ascending force, which appears to be almost sufficient to set aside the universal application of this law to the motions of fluids. It is confessedly demonstrable that each of two waves, crossing each other in any direction, will preserve its motion and its elevation with respect to the surface of the fluid affected by the other wave, in the same manner as if that surface were plane: and when the waves cross each other nearly in the same direction, both the height and the actual velocity of the particles being doubled, it is obvious that the ascending force or impetus is also doubled, since the bulk of the matter concerned is only halved, while the square of the velocity is quadrupled; and supposing the double wave to be stopped by an obstacle, its magnitude, at the moment of the greatest elevation, will be twice as great as that of a single wave in similar circumstances, and the height as well as the quantity of matter, will be doubled, so that either the actual or the potential height of the centre of gravity of the fluid seems to be essentially altered, whenever such an interference of waves takes place. This difficulty deserves the attentive consideration of those who shall attempt to investigate either the most refined parts of hydraulics, or the metaphysical principles of the laws of motion.

V.—*Of the Effect of a Contraction advancing through a Canal.*

If we suppose the end of a rectangular horizontal canal, partly filled with water, to advance with a given velocity, less than

that with which a wave naturally moves on the surface of the water, it may be shown that a certain portion of the water will be carried forwards, with a surface nearly horizontal, and that the extent of this portion will be determined, very nearly, by the difference of the spaces described, in any given time, by a wave, moving on the surface thus elevated, and by the moveable end of the canal. The form of the anterior termination of this elevated portion, or wave, may vary, according to the degrees by which the motion may be supposed to have commenced; but whatever this form may be, it will cause an accelerative force, which is sufficient to impart successively to the portions of the fluid, along which it passes, a velocity equal to that of the moveable end, so that the elevated surface of the parts in motion may remain nearly horizontal: and this proposition will be the more accurately true, the smaller the velocity of the moveable end may be. For calling this velocity v, the original depth a, the increased depth x, and the velocity of the anterior part of the wave y, we have, on the supposition that the extent of the wave is already become considerable, $x = \frac{ay}{y \mp v}$, taking the negative or positive sign according to the direction of the motion of the end; since the quantity of fluid, which before occupied a length expressed by y, now occupies the length $y \mp v$: and putting $a \backsim x = z$, $z = \frac{av}{y \mp v}$. The direction of the surface of the margin of the wave is indifferent to the calculation, and it is most convenient to suppose its inclination equal to half a right angle, so that the accelerating force, acting on any thin transverse vertical lamina, may be equal to its weight: then the velocity y must be such that while the inclined margin of the wave passes by each lamina, the lamina may acquire the velocity v by a force equal to its own weight; consequently the time of its passage must be equal to that in which a body acquires the velocity v, in falling through a height b, corresponding to that velocity: and this time is expressed by $\frac{2b}{v}$; but the space described by the margin of the wave is not exactly z, because the lamina in question has moved horizontally during its acceleration

through a space which must be equal to b; the distance actually described will therefore be $z \pm b$, and we have $\frac{z \pm b}{y} = \frac{2b}{v}$, $z \pm b = \frac{2by}{v}$, $av \pm by - bv = \frac{2byy}{v} \mp 2by$, $y^2 \mp \frac{3}{2}vy = \frac{av^2}{2b} - \frac{v^2}{2}$, $(y \mp \frac{3}{4}v)^2 = \frac{av^2}{2b} + \frac{v^2}{16}$; but, m being the proper coefficient, $v = m\sqrt{b}$, and $v^2 = m^2 b$, $\frac{av^2}{2b} + \frac{v^2}{16} = m^2\left(\frac{a}{2} + \frac{b}{16}\right)$, $y = m\sqrt{\left(\frac{a}{2} + \frac{b}{16}\right)} \pm \frac{3}{4}v$, and $y \mp v = m\sqrt{\left(\frac{a}{2} + \frac{b}{16}\right)} \mp \frac{1}{4}v$. But when v is small, we may take $y \mp v$ nearly $m\sqrt{\frac{a}{2}}$, and $z = \frac{ma\sqrt{b}}{m\sqrt{(\frac{1}{2}a)}} = \sqrt{(2ab)}$, and $x = a \pm \sqrt{(2ab)}$, while the height of a fluid, in which the velocity would be y, is nearly $a \pm \frac{3}{2}\sqrt{(2ab)}$: consequently, when the velocity v is at all considerable, y must be somewhat greater than the velocity of a wave moving on the surface of the elevated fluid; and probably the surface of the elevated portion will not in this case be perfectly horizontal; but where v is small, y may be taken, without material error, $m\sqrt{\frac{x}{2}}$, or even $m\sqrt{\frac{a}{2}}$, which is the velocity of every small wave. The coefficient m is here assumed the same for the motion of a wave, as for the discharge through an aperture, and I have reason from observation to think this estimation sufficiently correct.

Supposing now the moveable end of the canal to remain open at the lower part as far as the height c, then the excess of pressure occasioned by the elevation before it, and the depression behind, will cause the fluid, immediately below the moveable plane, to flow backwards, with the velocity determined by the height, which is the difference between the levels; and the quantity thus flowing back, together with that which is contained in the moveable elevation, must be equal to the whole quantity displaced. But the depression behind the moveable body must vary according to the circumstances of the canal, whether it be supposed to end abruptly at the part from which the motion begins, or to be continued backwards without limit: in the first case, the elevation z will be to the

depression, as v to $y.-v$, the length of the same portion of the fluid being varied inversely in that ratio; in the second case, the proportion will be as $y+v$ to $y-v$: and the difference of the levels will be first $z+z\frac{y-v}{v}=\frac{zy}{v}$, or secondly $z+z\frac{y-v}{y+v}=\frac{2xy}{y+v}$: and first, $m\sqrt{\frac{zy}{v}}c+(y-v)z=(a-c)v$; but, since y is here considered as equal to $m\sqrt{\frac{a}{2}}$, putting $\sqrt{\frac{a}{2}}-\sqrt{b}=d$, $y-v=md$, and, calling $a-c$, e, $m\sqrt{\frac{zy}{v}}c+mdz=me\sqrt{b}$, $\sqrt{\frac{zy}{v}}c+dz=e\sqrt{b}$, $c^2\frac{zy}{v}=e^2b+d^2z^2-2dze\sqrt{b}$, $z^2-\left(\frac{c^2y}{a^2v}+\frac{2e\sqrt{b}}{d}\right)z=-\frac{e^2b}{d^2}$, and, calling $\frac{c^2y}{2d^2v}+\frac{e\sqrt{b}}{d}$, f, $z=f-\sqrt{\left(f^2-\frac{e^2b}{d^2}\right)}$: and in the same manner f is found, for the second case, equal to $\frac{c^2y}{d^2(y+v)}+\frac{e\sqrt{b}}{d}$. For example, suppose the height a 2 feet, $b=\frac{1}{4}$, $c=1$, and consequently $e=1$, then d becomes $\frac{1}{2}$, $v=4$, and $y=8$; and in the first case $z=.1$, and in the second $z=.14$.

If v, the velocity of the obstacle, were great in comparison with $m\sqrt{\frac{a}{2}}$, the velocity of a wave, and the space c below the obstacle were small, the anterior part of the elevation would advance with a velocity considerably greater than the natural velocity of the wave: but if the space below the obstacle bore a considerable proportion to the whole height, the elevation z would be very small, since a moderate pressure would cause the fluid to flow back, with a sufficient velocity, to exhaust the greatest part of the accumulation, which would otherwise take place. Hence the elevation must always be less than that which is determined by the equation $m\sqrt{zc}=ev$, and z is at most equal to $\left(\frac{ev}{mc}\right)^2=\frac{e^2}{c^2}b$; but since the velocity of the anterior margin of the wave can never materially exceed $m\sqrt{\frac{x}{2}}$, especially when z is small, and $\sqrt{\frac{x}{2}}$ being in this case nearly $\sqrt{\frac{a}{2}}+\frac{e}{2\sqrt{(\frac{1}{2}a)c^2}}b$, $m\sqrt{\frac{x}{2}}-m\sqrt{b}=m\left(\sqrt{\frac{a}{2}}+\frac{e^2b}{\sqrt{(2a)c^2}}-\sqrt{b}\right)$

which, multiplied by z, shows the utmost quantity of the fluid that can be supposed to be carried before the obstacle. Supposing $b = \frac{1}{2} a$, this quantity becomes $m \sqrt{\frac{a}{2}} \cdot \frac{e^4}{c^4} \cdot \frac{a}{4}$; and if $\frac{e}{c}$ be, for example, $\frac{1}{10}$, it will be expressed by $\frac{1}{40000} av$.

A similar mode of reasoning may be applied to other cases of the propagation of impulses, in particular to that of a contraction moving along an elastic pipe. In this case, an increase of the diameter does not increase the velocity of the transmission of an impulse; and when the velocity of the contraction approaches to the natural velocity of an impulse, the quantity of fluid protruded must, if possible, be still smaller than in an open canal; that is, it must be absolutely inconsiderable unless the contraction be very great in comparison with the diameter of the pipe, even if its extent be such as to occasion a friction which may materially impede the retrograde motion of the fluid. The application of this theory to the motion of the blood in the arteries is very obvious, and I shall enlarge more on the subject when I have the honour of laying before the Society the Croonian Lecture * for the present year.

The resistance opposed to the motion of a floating body, might in some cases be calculated in a similar manner; but the principal part of this resistance appears to be usually derived from a cause which is here neglected; that is, the force required to produce the ascending, descending, or lateral motions of the particles, which are turned aside to make way for the moving body: while in this calculation their direct and retrograde motions only are considered.

The same mode of considering the motion of a vertical lamina may also be employed for determining the velocity of a wave of finite magnitude. Let the depth of the fluid be a, and suppose the section of the wave to be an isosceles triangle, of which the height is b, and half the breadth c: then the force urging any thin vertical lamina in a horizontal direction will be to its weight as b to c; and the space d, through which it moves horizontally, while half the waves pass it, will be such that $(c - d).(a + \frac{1}{2} b) = ac$, whence $d = \frac{bc}{2a + b}$. But the final

* No. XXIII., which follows.

velocity in this space is the same as is due to a height equal to the space, reduced in the ratio of the force to the weight, that is, to the height $\frac{bb}{2a+b}$, and half this velocity is $\frac{1}{2} m \sqrt{\left(\frac{bb}{2a+b}\right)}$, which is the mean velocity of the lamina. In the mean time the wave describes the space $c+d$, and its velocity is greater than that of the lamina in the ratio of $\frac{c}{d}+1$ to 1, that is $\frac{2a+b}{b}+1$ or $\frac{2a}{b}+2$ to 1, becoming $m\left(\frac{a}{b}+1\right)\frac{b}{\sqrt{(2a+b)}} = m \frac{a+b}{\sqrt{(2a+b)}}$; which, when b vanishes, becomes $m\sqrt{\frac{a}{2}}$, as in Lagrange's theorem, and, when b is small, $m\frac{a+\frac{1}{2}b}{\sqrt{(2a)}}$; but if a were small, it would approach to $m\sqrt{b}$, the velocity due to the whole height of the wave.

No. XXIII.

THE CROONIAN LECTURE.

ON THE FUNCTIONS OF

THE HEART AND ARTERIES.

From the Philosophical Transactions for 1809, p. 1.

READ NOV. 10, 1808.

THE mechanical motions, which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies. Thus the force of gravitation acts precisely in the same manner, and in the same degree, on living as on dead matter; the laws of optics are most accurately observed by all the refractive substances belonging to the eye; and there is no case in which it can be proved, that animated bodies are exempted from any of the affections to which inanimate bodies are liable, except when the powers of life are capable of instituting a process, calculated to overcome those affections, by others, which are commensurate to them, and which are of a contrary tendency. For example, animal bodies are incapable of being frozen by a considerable degree of cold, because animals have the power of generating heat; but the skin of an animal has no power of generating an acid, or an alkali, to neutralise the action of an alkaline or an acid caustic, and therefore its texture is destroyed by the chemical attraction of such an agent, when it comes into contact with it. As far, therefore, as the functions of animal life depend on the locomotions of the solids or fluids, those functions must be capable of being illustrated by the consideration of the mechanical laws of moving bodies; these laws being fully adequate to the explanation of the connexion between the motive powers, which are employed in the system, and the immediate effects, which they

are capable of producing, in the solids or fluids of the body: and it is obvious that the inquiry, in what manner, and in what degree, the circulation of the blood depends on the muscular and elastic powers of the heart and of the arteries, supposing the nature of those powers to be known, must become simply a question belonging to the most refined departments of the theory of hydraulics.

In examining the functions of the heart and arteries, I shall inquire, in the first place, upon the grounds of the hydraulic investigations which I have already submitted to the Royal Society, what would be the nature of the circulation of the blood, if the whole of the veins and arteries were invariable in their dimensions, like tubes of glass or of bone; in the second place, in what manner the pulse would be transmitted from the heart through the arteries, if they were merely elastic tubes; and in the third place, what actions we can with propriety attribute to the muscular coats of the arteries themselves. I shall lastly add some observations on the disturbances of these motions which may be supposed to occur in different kinds of inflammations and of fevers.

When we consider the blood-vessels as tubes of invariable dimensions, we may suppose, in order to determine the velocity of the blood in their different parts, and the resistances opposed to its motion, that this motion is nearly uniform, since the alternations, arising from the pulsation of the heart, do not materially affect the calculation, especially as they are much less sensible in the smaller vessels than in the larger ones, and the principal part of the resistance arises from these small vessels. We are to consider the blood in the arteries as subjected to a certain pressure, by means of which it is forced into the veins, where the tension is much less considerable; and this pressure, originating from the contraction of the heart, and continued by the tension of the arteries, is almost entirely employed in overcoming the friction of the vessels: for the force required to overcome the inertia of the blood is so inconsiderable, that it may, without impropriety, be wholly neglected. We must therefore inquire, what the magnitude of this pressure is, and what degree of resistance we can suppose to arise from the

friction of the internal surface of the blood-vessels, or from any other causes of retardation. The magnitude of the pressure has been ascertained by Hales's most interesting experiments on a variety of animals, and may thence be estimated with sufficient accuracy for the human body; and for determining the magnitude of the resistance, I shall employ the theorems which I have deduced from my own experience on very minute tubes, compared with those which had been made by former observers under different circumstances; together with some comparative experiments on the motion of water and of other fluids in the same tubes.

Dr. Hales infers, from his experiments on quadrupeds of different sizes, that the blood in the human arteries is subjected to a pressure, which is measured by a column of the height of seven feet and a half: in the veins, on the contrary, the pressure appears to amount to about six inches only; so that the force which urges the blood from the greater arteries through the minuter vessels into the large veins, may be considered as equivalent to the pressure of a column of seven feet.

In order to calculate the magnitude of the resistance, it is necessary to determine the dimensions of the arterial system, and the velocity of the blood which flows through it. According to the measurements of Keill and others, we may take $\frac{3}{4}$ of an inch for the usual diameter of the aorta, and suppose each arterial trunk to be divided into two branches, the diameter of each being about $\frac{1}{2}$ of that of the trunk (or more accurately $1 : 1.26 = 10 - .100567$), and the joint areas of the sections about a fourth part greater (or $1.2586 : 1 = 10^{.099896}$). This division must be continued twenty-nine times, so that the diameter of the thirtieth segment may be only the eleven hundredth part of an inch, that is nearly large enough to admit two globules of the blood to pass at once. The length of the first segment must be assumed about nine inches, that of the last, the twentieth of an inch only; and supposing the lengths of the intermediate segments to be a series of mean proportionals, each of them must be about one-sixth part shorter than the preceding (or $1 : 1.961 = 10 - .07776$), the mean length of the whole forty-six inches, the capacity to

that of the first segment as 72.71 to 1, and consequently the weight of the blood contained in the arterial system about 9.7 pounds. It is probable that this calculation approaches sufficiently near to the truth; for the whole quantity of blood in the body being about 40 pounds, although some have supposed it only 20, others no less than 100, there is reason to believe that half of this quantity is contained in the veins of the general circulation, and that the other half is divided, nearly in equal proportions, between the pulmonary system and the remaining arteries of the body, so that the arteries of the general circulation may contain about 9 or 10 pounds. Haller allows 50 pounds of circulating fluid, partly serous, and partly red, and supposes $\frac{1}{5}$ of this to be contained in all the arteries taken together: but in a determination which must be in great measure conjectural we cannot expect perfect accuracy: and according to Haller's own account of the proportions of the sections of the arteries and veins, the large trunks of the veins appear to be little more than twice as capacious as those of the arteries, and the smaller branches much more nearly equal, so that we cannot attribute to the arterial system less than $\frac{1}{3}$ of the whole blood.

It may be supposed that the heart throws out, at each pulsation, that is about seventy-five times in a minute, an ounce and a half of blood: hence the mean velocity in the aorta becomes eight inches and a half in a second: and the velocity in each of the succeeding segments must of course be smaller, in proportion as the joint areas of all the corresponding sections are larger than the area of the aorta: for example, in the last order of vessels, of which the diameter is the eleven hundredth of an inch, the velocity will be one ninety-third of an inch; and this result agrees sufficiently well with Hales's observation of the velocity in the capillary arteries of a frog, which was one-ninetieth part of an inch only. It is true, that Haller is disposed to question the accuracy of this observation, and to attribute a much greater velocity to the blood flowing through the capillary vessels, but he did not attempt either to measure the velocity, or to determine it by calculation: nor is this the only instance in which Haller has been

led to reason erroneously, from a want of mathematical knowledge: he may, however, have observed the particles of blood moving in the axis of a vessel with a velocity much exceeding the mean velocity of its whole contents. If we calculate upon these foundations, from the formula which I have already laid before the Society, it will appear that the resistance which the friction of the arteries would occasion, if water circulated in them instead of blood, with an equal velocity, must amount to a force equivalent to the pressure of a column of fifteen inches and a half: to this we may add about a fourth for the resistance of the capillary veins, and we may estimate the whole friction for water, at twenty inches. The only considerable part of this force is derived from the term $\frac{2.1126\,lv}{10^7 d^{3.5}}$ * in the value of f: this term increases for each successive segment in the ratio $1 : 1.49425 = 1 : n$, and the sum of the series is to the first term, as $\frac{n^{30}-1}{n-1}$ to 1. It appears also, that a very small portion only of the resistance is created in the larger vessels: thus, as far as the twentieth division, at the distance of an inch and a quarter only from the extreme capillary arteries, the pressure of a column of one-twentieth of an inch only is required for overcoming the whole friction, and at the twenty-fifth division, where the artery does not much exceed the diameter of a human hair, the height to which the water would rise, in a tube fixed laterally into the artery, is only two inches less than in the immediate neighbourhood of the heart.

In order to judge of the comparative resistance produced by fluids of different degrees of viscidity, I employed the same tubes, by means of which I had determined the friction of water, in extreme cases, for ascertaining the effect of different substances held in solution in the water: since it is impossible to make direct experiments on the blood in its natural state, on account of its tendency to coagulate: and those substances which have the power of preventing its coagulation, may naturally be supposed to produce a material change in its viscidity. The diameter of one of the tubes, which was cylindrical, was

* Supra, p. 493. The expression for the friction referred to does not contain this term.—*Note by the Editor.*

the fortieth part of an inch: the bore of the other was oval, as is usual in the finest tubes made for thermometers: the section divided by one-fourth of the circumference, gave one hundred and seventy seconds for the mean diameter. I caused some milk, and solutions of sugar of different strength, to pass through these tubes: they were all transmitted much more sparingly than water, with an equal pressure, and the difference was more considerable in the smaller than in the larger tube, as might naturally be expected both from the nature of the resistance, and from the result of Gerstner's experiments on water at different temperatures. In the first tube the resistance to the motion of milk was three times as great as that of water, a solution of sugar in five times its weight of water produced twice as much resistance as water; in twice its weight, nearly four times as much as water: but in the narrower tube, the weaker solution of sugar exhibited a resistance five times as great as that of water, which is more than twice as much as appeared in the larger tube. Hence there can be no doubt that the resistance of the internal surface of the arteries to the motion of the blood must be much greater than would be found in the case of water: and supposing it about four times as great, instead of 20 inches, we shall have 80, for the measure of a column of which the pressure is capable of forcing the blood, in its natural course, through the smaller arteries and veins, which agrees very well with Hales's estimate.

This determination of the probable dimensions of the arterial system, and of the resistances occasioned by its different parts, is in some few respects arbitrary, at the same time that it cannot be materially altered without altering either the whole quantity of blood contained in the body, the diameters of the smallest capillary vessels, the mean number of bifurcations, or the magnitude of the resistance, all of which are here assumed nearly as they have been laid down by former observers: the estimation of the length of the successive segments only is made in such a manner as to reconcile these data with each other, by means of the experiments and calculations relating to the friction of fluids in pipes. The effect of curvature in increasing the resistance has been hitherto neglected; it can be only

sensible in the larger vessels; and supposing the flexures of these to be equivalent to the circumferences of two circles, each two inches in diameter, the radius q being 1, we have $r = \frac{.0000045p\ v^2 q^{\frac{1}{6}}}{q} = .0000045 \times 720 \times 64 = .207$, or about one-fifth of an inch, for the additional resistance arising from this cause in the case of water, or four-fifths for blood, which is a very inconsiderable part of the whole.

It might be questioned whether the experiments which I have made, with tubes $\frac{1}{17}$ of an inch in diameter, are sufficient for determining with accuracy the degree in which the resistance would be increased in tubes, of which the diameter is only one-sixth part as great; and it may be doubted whether the analogy, derived from these experiments, can be safely employed as a ground for asserting, that so large a portion of the arterial pressure is employed in overcoming the resistance of the very minute arteries. But it must be remembered, that these experiments are at least conclusive with respect to the arteries larger than the tubes employed in them, and even those which are a little smaller; so that the remaining pressure, as observed in experiments, can only be employed in overcoming the resistance of the minuter arteries and veins, and these observations tend therefore immediately to confirm the analogy drawn from the experiments on the motion of water. It might indeed be asserted that the viscidity of the blood exceeds that of water in a much greater ratio than that which is here assigned; but this is rendered improbable by some experiments of Hales, in which, when the intestines were laid open, on the side opposite to the mesentery, so that many of the smaller arteries were divided, the quantity of warm water which passed through them with an equal pressure, was only about twelve times as great as that of the blood which flows through them in their natural state; and it is probable that at least three or four times as much of any fluid must have passed through them in their divided, as in their entire state, unless we suppose that the coats of the divided vessels, like many other muscular parts, are capable of being contracted by the contact of water. In some other experiments, it was found that a moderate degree of pres-

sure was capable of causing water to exude so copiously through the exhalant vessels of the intestines, that it passed through the aorta with a velocity of about two inches in a second, although these vessels do not naturally allow any passage to the blood: on the other hand, it sometimes happened that very little water would pass through such channels as naturally transmitted a much larger quantity of blood: a circumstance which Dr. Hales very judiciously attributes to the oozing of the water into the cellular membrane surrounding the vessels, by means of which they were compressed, and their diameters lessened. On the whole, it is not improbable that, in some cases, the resistance, opposed to the motion of the blood, may exceed that of water in a ratio somewhat greater than I have assigned; but this must be in the minutest of the vessels, while in the larger arteries the disproportion must be less: so that, however we may view the subject, it appears to be established, that the only considerable resistance which the blood experiences, occurs in the extreme capillary arteries, of which the diameter scarcely exceeds the hundredth part of an inch.

We cannot suppose that the dimensions of the sanguiferous system agree uniformly, in all its parts, with the measures which I have laid down; but the truth of the inference is not affected by these variations. For example, there may perhaps be some arteries communicating with veins, of which the diameter exceeds the eleven-hundredth of an inch; but there are certainly many others which are much more minute; and the blood, or its more liquid parts, passing through these more slowly, it must move more rapidly in the former, so that the resistance may in all be equal to the pressure, and the mean velocity may still remain such as is determined by the quantity of blood passing through the aorta. There is indeed some uncertainty in the measure of the globules of the blood, which I have made the basis of the dimensions of the minute arteries: and I have reason to think, that instead of $\frac{1}{100}$ of an inch, their greatest diameter does not exceed $\frac{1}{200}$, or even $\frac{1}{300}$: the general results of the investigation are not however affected by this difference: it will only require us to suppose the subdivisions somewhat more numerous, and the branches shorter.

These are the principal circumstances which require to be considered, with respect to the simple transmission of the blood through the arteries into the veins, without regard to the alternate motions of the heart, and to the elastic and muscular powers of the vessels. I shall next examine the nature and velocity of the propagation of the pulse. The successive transmission of the pulsations of the heart, through the length of the arteries, is so analogous to the motion of the waves on the surface of water, or to that of a sound transmitted through the air, that the same calculations will serve for determining the principal affections of all these kinds of motion; and if the water, which is agitated by waves, is supposed to flow at the same time in a continued stream, and the air which conveys a sound to be carried forwards also in the form of a wind, the similitude will be still stronger. The coats of the arteries may perhaps be considered, without much inaccuracy, as perfectly elastic, that is, as producing a force proportional to the degree in which they are extended beyond their natural dimensions; but it is not impossible that there may be some bodies in nature, which differ materially from this general law, especially where the distension becomes considerable: thus there may be substances which exhibit a force of tension proportional to the excess of the square, or the cube of their length, beyond a certain given quantity. It is safest therefore to reason upon the elasticity of any substance, from experiments made without any great deviation from the circumstances to which the calculation is to be applied.

For this purpose, we may again employ some of the many excellent experiments contained in Hales's hæmastatics. It appears, that when any small alteration was made in the quantity of blood contained in the arteries of an animal, the height of the column, which measured the pressure, was altered nearly in the same proportion, as far as we are capable of estimating the quantity, which was probably contained in the larger vessels of the animal. Hence it follows, that the velocity of the pulse must be nearly the same as that of an impulse transmitted through an elastic tube, under the pressure of a column of the same height, as that which measures the actual arterial pres-

sure: that is, equal to that which is acquired by a heavy body falling freely through half this height. In man, this velocity becomes about fifteen feet and a half in a second; to which the progressive motion of the blood itself adds about eight inches; and with this velocity, of at least sixteen feet in a second, it may easily happen that the pulse may appear to arrive at the most distant parts of the body without the intervention of any very perceptible interval of time.

The velocity of the transmission of the pulse being known, it is easy to determine the degree in which the arteries are dilated during its passage through them. The mean velocity of the blood in the aorta being eight inches and a half in a second, its greatest velocity must be about three times as much, since the contraction of the heart is supposed to occupy only about one-third part of the interval between two successive pulsations; and if the velocity of the pulse is sixteen feet in a second, that of the blood itself must be about one-eighth part as great; so that the column of blood occupying eight inches may occupy only seven; hence the diameter must increase in the ratio of about fifteen to sixteen. The tension will also become one-eighth greater, and the force of the heart must be capable of supporting a column of one hundred and one inches. This force would, however, require to be somewhat increased, from the consideration that the force required at the end of any canal during the reflection of a pulsation or wave of any kind, is twice as great as the force exerted during its transmission, and the force employed in the origination of a wave or pulse in a quiescent fluid, is the same as is required for its reflection; on the other hand, a weaker pulsation, proceeding into a narrower channel, becomes more energetic, so that, from this consideration, a force somewhat smaller would be required in the heart: on the whole, however, it appears probable, that the former of these corrections must be the more considerable, and that the force of the heart must be measured by the pressure of a column, rather more than less than one hundred and one inches high; nor would this force by any means require a strong exertion of muscular power; for it only implies a tension of something less than three pounds for each inch of the circum-

ference of the greatest section of the heart; and supposing the mean thickness half an inch, an equal number of the fibres of some other muscles of the body would be capable of exerting a force of more than two hundred pounds, in the state of the greatest possible action.

The force, here assigned to each pulsation, agrees extremely well with the inference that may be drawn from an experiment of Hales on the ascent of the blood in a tube connected with an artery of a horse. The whole height of the column being nine feet, the blood rose about three inches higher during each pulsation, which was repeated fifty or sixty times in a minute: now we may suppose the acceleration to have extended a little beyond the first half of the space thus described, so that two inches were described in two-fifths of a second; and if there had been no friction, nor any other cause of retardation, there can be no doubt that at least four inches would have been described in the same time; but the same column of nine feet, if it had been actuated by its own weight, would have described thirty-one inches in the same time; consequently the force with which the blood was driven through the artery was nearly one-eighth of the whole force of tension, as it appears in the former calculation.

The magnitude of the pulse must diminish in the smaller arteries in the subduplicate proportion of the increase of the joint areas, in the same manner as the intensity of sound is shown to decrease in diverging from a centre, in the subduplicate ratio of the quantity of matter affected by its motion at the same time. For example, in the arteries of the tenth order, of which the diameter is one-thirteenth of an inch, its magnitude must be only one-third as great as in the aorta, that is, the greatest progressive velocity of the blood must be eight inches and a half in a second only, and the dilatation one-fiftieth part only of the diameter. In the vessels of the twentieth order, the dilatation does not exceed $\frac{1}{1000}$ of the diameter, which is itself the 140th part only of an inch; so that it is not surprising that Haller should have been unable to discover any dilatation in vessels of these dimensions, even with the assistance of a powerful microscope. If we estimated the magnitude of the pulse in

the aorta, from the excess of the temporary above the mean velocity, which would perhaps be justifiable, that magnitude would become still less considerable.

These calculations agree extremely well with each other, and with experiment, as far as they relate to the power of the heart, and the affections of the smaller arteries. But there is reason to think that the velocity of the pulse in the larger vessels is much more considerable than has been here stated; and their dilatation is also less conspicuous, when they are exposed to view, than it would probably be, if it were as great as is inferred from the velocity here assigned. I have demonstrated, in the hydraulic investigations which I lately laid before the Royal Society, that the velocity of an impulse passing through a tube, consisting of perfectly elastic materials, is half as great as that of a body supposed to have fallen from the given point to the base of the modular column of the tube: and that the height of this column is such that the tube would be extended without limit by its pressure; consequently it must be greater than the height of a column equivalent to the pressure by which the tube is burst. Now it has been ascertained by Dr. Hales, that the pressure, required for bursting one of the carotids of a dog, is equal to that of a column of water one hundred and ninety feet high; nor does he remark that the artery was very materially dilated; and deducting from this height the five feet which express the actual pressure in the arteries of a dog, the remaining one hundred and eighty-five feet will give a velocity of at least fifty-four feet in a second, for the propagation of the pulse in the dog. It is not however ascertained, that all the membranes, which may have surrounded the artery in this experiment, are called into action in its ordinary pulsation, much less that the force, developed by their tension, varies precisely according to the general law of perfectly elastic bodies: but this mode of calculation is still amply sufficient to make it probable that the velocity of the pulsations, in the larger arteries, must amount to at least forty feet in a second, although some very considerable deductions must be made, on account of the resistances of various kinds, which cannot be comprehended in the calculation.

The artery must not be supposed to subside, immediately after each pulsation, precisely to its original dimensions, since it must remain somewhat fuller, in order to supply the capillary arteries, and the veins, in the interval between the two successive pulsations: and in this respect it differs from the motions of a wave through a canal which is open on both sides: but the difference may be understood, by supposing a partial reflection of the pulse to take place at every point where it meets with any resistance, which will leave a general distension of the artery, without any appearance of a retrograde pulsation.

I shall proceed to inquire, in the third place, into the nature and extent of the functions which are to be attributed to the muscular fibres of the coats of the arteries; and I apprehend that it will appear to be demonstrable, that they are much less concerned in the progressive motion of the blood, than is almost universally believed. The arguments which may be employed to prove this, are nearly the same that I have already stated, in examining the motion of a fluid, carried along before a moving body in an open canal; but in the case of an elastic tube, the velocity of the transmission of an impulse being rather diminished than increased by an increase of tension, the reasoning is still stonger and simpler; for it may here be safely asserted, that the anterior parts of the dilatation, which must be forced along by any progressive contraction of the tube, can only advance with the velocity appropriate to the tube, and that its capacity must be proportionate to its length and to the area of its section: now the magnitude of its section must be limited by that degree of tension which is sufficient to force back through the contraction what remains of the displaced fluid, and the length by the difference of the velocity appropriate to the tube, and that with which the contraction advances; consequently if the contraction advance with the velocity of a pulsation, as any contractile action of the arteries must be supposed to do, this length necessarily vanishes, and with it the quantity of the fluid protruded; the whole being forced backwards, by the distending force which is exerted by a very small dilated portion, immediately preceding

the contraction. It might indeed be imagined, that the contraction follows the pulsation with a velocity somewhat smaller than its own; but this opinion would stand on no other foundation than mere conjecture, and it would follow, that the pulse would always become more and more full, as it became more distant from the heart; of which we have nothing like evidence: nor would a moderate contraction, even if this supposition were granted, produce any material effect. For example, if the velocity of the contraction were only half as great as that of the pulsation, which is the most favourable proportion, it would be necessary, taking sixteen feet in a second for the velocity of the pulsation, that the section of the arteries should be contracted to about one-half, in order to produce, by their progressive contraction only, the actual velocity of the blood in the aorta; one-sixteenth of the blood being carried, in this case, before the contraction: but if the contraction were only such, as to reduce the section of the artery to $\frac{9}{10}$, which is probably more than ever actually happens, the velocity produced would be only about $\frac{1}{60}$ as much; and if the contraction were only to $\frac{9}{10}\frac{9}{10}$, which is a sufficient allowance for the smaller arteries, about $\frac{1}{1000}$ only of the actual velocity in the aorta could be produced in this manner, even upon a supposition much more favourable to the muscular action of the arteries than the actual circumstances. A small addition must be made to the force required for producing the retrograde motion, on account of the friction to be overcome, but the general reasoning is not affected by this correction.

The contraction of the artery might also be supposed to remain after each pulsation, so that the vessel should not be again dilated until the next pulsation, or, in other words, a spontaneous dilatation might be supposed to accompany the pulsation, instead of a contraction: but such a dilatation would be useless in promoting the progressive motion of the blood, since a larger quantity of blood, conveyed to the smaller vessels, without an increased tension, would be ineffectual with respect to the resistances which are to be overcome. It is possible indeed that the muscular fibres of those arteries in which the magnitude of the pulse is sensible, like the fibres of

the heart, may be inactive, or nearly so, during their dilatation, and that they may contract after they have been once distended, with a force which is in a certain degree permanent; the greater momentum of the blood, which accompanies the dilatation, enabling it to enter the minute arteries with equal ease, although assisted by a tension somewhat smaller: so that the same mean velocity may be sustained, as if the arteries were simply elastic, and a little smaller in diameter, with a very little less exertion of the heart. But the distribution of the blood could never be materially diversified by any operation of this kind: for if any artery were for a moment distended by such a variation, so as to exceed its natural diameter by one hundredth part only, a pressure would thence arise equivalent to that of a column about two inches high, which would, in spite of all resistances, immediately dissipate the blood with a considerable velocity, and completely prevent any local accumulation, unless the elastic powers of the vessel itself were diminished; and this is, perhaps, the most important, as well as the best established inference from the doctrine that I have advanced.

It appears that a mola has sometimes been found in the uterus, totally destitute of a heart, in which the blood must have circulated in its usual course through the veins and arteries: in this case it cannot be ascertained whether there was any alternate pulsation, or whether the blood was carried on in a uniform current, in the same manner as the sap of a vegetable probably circulates. If there was a pulsation, it may have been maintained by a contraction of the artery, much more considerable, and slower in its progress than usual; and with the assistance of a spontaneous dilatation; the resistance in the extreme vessels being also probably much smaller than usual: if the motion was continued, it would lead us to imagine that there may be some structure in the placenta capable of assisting in the propulsion of the blood, as there may possibly be some arrangement in the roots of plants by which they are calculated to promote the ascent of the sap. The circulation in the vessels of the more imperfect animals, in which a great artery supplies the place of a heart, is of a very different na-

ture from that of the more perfect animals: the great artery, which performs the office of the heart, is here possessed of a muscular power commensurate to its functions, and seems to propel the blood, though much more slowly than in other cases, by means of a true peristaltic motion. It appears also from the observations of Spallanzani, that in many animals a portion of the aorta, next the heart, is capable of exhibiting a continued pulsation, even when perfectly empty and separated from the heart; but this property is limited to a small part of the artery only, which is obviously capable of being essentially useful in propelling the blood when the valves of the aorta are closed. The muscular power of the termination of the vena cava is also capable of assisting the passage of the blood into the auricle. It is not at all improbable that a muscle of involuntary motion, which had been affected throughout the whole period of life by alternate contractions and relaxations, might retain from habit the tendency to such contractions, even without the necessity of supposing that the habit was originally formed for a purpose to be obtained by the immediate exertion of the muscular power: but in fact the partial pulsation of the vena cava is perfectly well calculated to promote the temporary repletion of the auricle, while it must retard, for a moment, the column which is approaching, at a time that it could not be received.

There is no difficulty in imagining what services the muscular coats of the arteries may be capable of performing, without attributing to them any immediate concern in supporting the circulation. For since the quantity of blood in the system is on many accounts perpetually varying, there must be some means of accommodating the blood-vessels to their contents. This circumstance was very evident in some of Hales's experiments, when after a certain quantity of blood had been taken away, the height of the column, which measured the tension of the vessels, frequently varied in an irregular manner, before it became stationary at a height proportional to the remaining permanent tension. Haller also relates, that he has frequently seen the arteries completely empty, although in some of his observations there was probably only a want of red globules in

the blood which was flowing through them. Such alterations in the capacity of the different parts of the body are almost always to be attributed to the exertion of a muscular power. A partial contraction of the coats of the smaller arteries may also have an immediate effect on the quantity of blood contained in any part, although very little variation could be produced in this manner by a change of the capacity of the larger vessels.

According to this statement of the powers which are concerned in the circulation, it must be obvious that the nature of the pulse, as perceptible to the touch, must depend almost entirely on the action of the heart, since the state of the arteries can produce very little alteration in its qualities. The greater or less tension of the arterial system may indeed render the artery itself, when at rest, somewhat harder or softer; and, if the longitudinal fibres give way to the tending force, it may become also tortuous: possibly too a very delicate touch may in some cases perceive a difference in the degree of dilatation, although it is seldom practicable to distinguish the artery, in its quiescent state, from the surrounding parts. But the sensation, which is perceived when the artery is compressed, as usual, by the finger, is by no means to be confounded with the dilatation of the artery; for in this case an obstacle is opposed to the motion of the blood, against which it strikes with the momentum of a considerable column, almost in the same manner as a stream of water strikes on the valve of the hydraulic ram; and in this manner, neglecting the difference of force arising from the different magnitudes of the sections, the pressure felt by the finger becomes nearly equal and similar to that which is originally exerted by the heart: each pulsation passing under the finger, in the same time, as is required for the contraction of the heart, although a very little later; and more or less so, in proportion as the artery is more or less distant; the artery remaining then at rest for a time equal to that in which the heart is at rest. When therefore an artery appears to throb, or to beat more strongly than usual, the circumstance is only to be explained from its greater dilatation, which allows it to receive a greater portion of the action of the heart, in the

same manner as an aneurism exhibits a very strong pulsation, without any increase of energy, either in itself, or in the neighbouring vessels; and on the other hand, when the pulsations of the artery of a paralytic arm become feeble, we cannot hesitate to attribute the change to its permanent contraction, since the enlargement and contraction of the blood-vessels of a limb are well known to attend the increase or diminution of its muscular exertions. There is also another way, in which the diminution of the strength of an artery may increase the apparent magnitude of the pulse, that is, by diminishing the velocity with which the pulsation is transmitted: for we have seen that the magnitude of the pulse is in the inverse ratio of the length of the artery distended at once; and this length is proportional to the velocity of the transmission; but it must be observed, that the force of the pulse striking the finger would not be affected by such a change, except that it might be rendered somewhat fuller and softer, although a considerable throbbing might be felt in the part, from the increased distension of the temporary diameter of the artery. How little a muscular force is necessary for the simple transmission of a pulsation, may easily be shown by placing a finger on the vena saphena, and striking it with the other hand at a distant part; a sensation will then be felt precisely like that of a weak arterial pulsation.

The deviations from the natural state of the circulation, which are now to be cursorily investigated, may be either general or partial; and the general deviations may consist either in a change of the motion of the heart, or of the capacity of the capillary arteries. When the motion of the heart is affected, the quantity of blood transmitted by it may either remain the same as in perfect health, or be diminished, or increased. Supposing it to remain the same, the pulse, if more frequent, must be weaker, and if slower, it must be stronger; but this latter combination is scarcely ever observable; and in the former case, the heart must either never be filled, perhaps on account of too great irritability, or never be emptied, from the weakness of its muscular powers. But the immediate effect of such a change as this, in the functions depending on the circulation, cannot be very material, and it can only be considered

as an indication of a derangement in the nervous and muscular system, which is not likely to lead to any disease of the vital functions. When the quantity of the blood transmitted by the heart is smaller than in health, the arteries must be contracted, until their tension becomes only adequate to propel the blood, through the capillary vessels, with a proportionally smaller velocity, and the veins must of course become distended, unless the muscular coats of the arteries can be sufficiently relaxed to afford a diminished tension, which is probably possible in a very limited degree only. In this state the pulse must be small and weak, and the arteries being partly exhausted, there will probably be a paleness and chilliness of the extremities: until the blood, which is accumulated in the veins, has sufficient power to urge the heart to a greater action, and perhaps from the vigour which it may have acquired during the remission of its exertions, even to a morbid excess of activity. Hence a contrary state may arise, in which the quantity of blood transmitted by the heart is greater than in perfect health; the pulse will then be full and strong, the arteries being distended, so as to be capable of exerting a pressure sufficient to maintain an increased velocity, and to overcome the consequent increase of resistance; a state which perhaps constitutes the hot fit of fever; and which is probably sometimes removed in consequence of a relaxation of the extreme arteries, which suffer the superfluous blood to pass more easily into the veins. Such a relaxation, when carried to a morbid extent, may also be a principal cause of another general derangement of the circulation, the motion of the blood being accelerated, and the arteries emptied, so that the pulse may be small and weak, while the veins are overcharged, and the heart exhausted by violent and fruitless efforts to restore the equilibrium; and this state appears to resemble, in many respects, the affections observed in typhus. When, on the contrary, the capillary vessels are contracted, the arteries are again distended, although without the excess of heat which must attend their distension from an increased action of the heart, and possibly without fever: an instance of this appears to be exhibited in the shrinking of the skin, which is frequently observable from the effect of cold, and in the first impression

produced by a cold bath: nor is it impossible, that such a contraction may exist in the cold fit of an intermittent, although it seems more probable that a debility of the heart is the primary cause of this affection.

Besides these general causes of derangement, which appear to be more or less concerned in different kinds of fever, there are other more partial ones, which seem to have a similar relation to local inflammations. The most obvious of these changes are such as must be produced by partial dilatations or contractions of the capillary vessels: since, as I have endeavoured to demonstrate, any supposed derangement in the actions of the larger vessels must be excluded from the number of causes which can materially affect the circulation. It cannot be denied, that a diminution of the elastic, or even of the muscular force of the small arteries, must be immediately followed by such a distension as will produce a resistance equal to the pressure: the distension will occasion an increase of redness, and in most cases pain: the heat will also generally be increased, on account of the increased quantity of blood which will be allowed to pass through the part; and since the hydrostatic pressure of the blood acquires greater force, as the artery becomes more distended, it may be so weak as to continue to give way, like a ligament which has been strained, until supported by the surrounding parts. In this state a larger supply of blood will be ready for any purposes which require it, whether an injury is to be repaired, or a new substance formed; and it is not impossible, that this change in the state of the minute vessels may ultimately produce some change in the properties of the blood itself.

The more the capillary arteries are debilitated and distended the greater will be the mean velocity of the circulation; but whether or no the velocity will be increased in the vessels which are thus distended, must depend on the extent of the affected part; and it may frequently happen that the velocity may be much more diminished on account of the dilatation of the space which the blood is to occupy, than increased by the diminution of the resistance. And on the other hand, the velocity may be often increased, for a similar reason, at the place of a partial

contraction. Hence we may easily understand some of the experiments which Dr. Wilson has related in his valuable treatise on fevers; the application of spirit of wine to a part of the membrane of a frog's foot contracted the capillary arteries, and at the same time accelerated the motion of the blood in them, while in other parts, where inflammation was present, and the vessels were distended, the motion of the blood was slower than usual.

Another species of inflammation may probably be occasioned by a partial constriction or obstruction of the capillary arteries, which must indeed be supposed to exist where the blood has become wholly stagnant, as Dr. Wilson in some instances found it. This obstruction must however be extended to almost all the branches, belonging to some small trunk, in which the pressure remains nearly equal to the tension of the large arteries: for in this case it will happen, that the whole pressure will be continued throughout the obstructed branches, without the subtraction of the most considerable part, which is usually expended in overcoming the resistances dependent on the velocity; so that the small branches will be subjected to a pressure, many times greater than that which they are intended to withstand in the natural state of the circulation; whence it may easily happen that they may be morbidly distended; and this distension may constitute an inflammation, attended by redness and pain. Nor is it impossible that obstructions of this kind may originate in a vitiated state of the blood itself, although it would be difficult to prove the truth of the conjecture; it seems, however, to be favoured by the observation of Haller, that little clots of globules may often be observed in the arteries, when the circulation is languid, and that they disappear when its vigour is restored, especially after venesection. But if a very small number only of capillary arteries be obstructed, other minute branches will still be capable of receiving the blood, which ought to pass through them, without any great distension or increase of pressure: and this exception is sufficient to explain another experiment of Dr. Wilson, in which a small obstruction, caused by puncturing a membrane with a hot needle, failed to excite an inflammation. This species of

inflammation is probably attended by less heat than the former; and where the obstruction is very great, it may perhaps lead immediately to a mortification, which is called by the Germans "a cold burning."

The most usual causes of inflammation appear to be easily reconcileable with these conjectures. Suppose any considerable part of the body to be affected by cold; the capillary vessels will be contracted, and at the same time the temperature of some parts of their contents will be lowered, from both of which causes the resistance will be increased, and the arteries in general will be more or less overcharged: if then any other part of the system be at the same time debilitated or overheated, its arteries will be liable to be morbidly distended, and an inflammation may thus arise, which may continue till the minute vessels are supported and strengthened, by means of an effusion of coagulable lymph. The immediate effect, either of cold or of heat, may also sometimes produce such a degree of debility in any part, as may lay the foundation of a subsequent inflammation: but the first effect of heat in the blood-vessels appears to be the more ready transmission of the blood into the veins, by means of which they become very observably prominent: and cold, which checks the circulation in the cutaneous vessels, probably occasions a livid hue, by retaining the blood stagnant longer than usual in the capillary vessels of all kinds. It may be objected, that an obstruction of the motion of the blood through a great artery ought, upon these principles, to produce an inflammation in some distant part: but in this case, the blood will still find its way very copiously into the parts supplied by the artery, by means of some collateral branches, which will always admit a much larger quantity of blood than usually passes through them, whenever a very slight excess of force can be found to carry it on, or when the blood which they contain finds a readier passage than usual, by means of their communication with such parts as are now deprived of their natural supply.

It is difficult to determine, whether blushing is more probably effected by a constriction or by a relaxation of the vessels concerned; it must, however, be chiefly an affection

of the smaller vessels, since the larger ones do not contain a sufficient quantity of blood to produce so sudden an effect. Perhaps the capillary vessels are dilated, while the arteries, which are a little larger only, are contracted: possibly too an obstruction may exist at the point of junction of the arteries with the veins; and where the blush is preceded by paleness, such an obstruction is probably the principal cause of the whole affection.

With respect to the tendency of inflammation in general to extend itself to the neighbouring parts, it is scarcely possible to form any reasonable conjecture that can lead to its explanation: this circumstance appears to be placed beyond the reach of any mechanical theory, and to belong rather to some mutual communication of the functions of the nervous system, since it is not inflammation only, that is thus promulgated, but a variety of other local affections of a specific nature, which are usually complicated with inflammation, although they may, perhaps, in some cases, be independent of it. Inflammations, however, are certainly capable of great diversity in their nature, and it is not to be expected, that any mechanical theory can do more than afford a probable explanation of the most material circumstances, which are common to all the different species.

Besides these general illustrations of the nature of fevers and inflammations, the theory which has been explained may sometimes be of use, in enabling us to understand the operation of the remedies employed for relieving them. Thus it may be shown, that any diminution of the tension of the arterial system must be propagated from the point at which it begins, as from a centre, nearly in the same manner, and with the same velocity, as an increase of tension, or a pulsation of any kind would be propagated. Hence the effect of venesection must be not only more rapidly, but also more powerfully felt in a neighbouring than in a distant part; and although the mean or permanent tension of the vessels of any part must be the same, from whatever vein the blood may have been drawn, provided that they undergo no local alteration, yet the temporary change, produced by opening a vein in their neighbourhood, may have relieved them so effectually from an excess of pressure, as to

allow them to recover their natural tone, which they could not have done without such a partial exhaustion of the neighbouring vessels. But since it seems probable, that the minute arteries are more affected by distension than the veins, there is reason in general to expect a more speedy and efficacious relief in inflammations, from opening an artery than a vein: this operation, however, can seldom be performed without material inconvenience; but it is probably for a similar reason, that greater benefit is often experienced from withdrawing a small portion of blood by means of cupping or of leeches, than a much larger quantity by venesection, since both the former modes of bleeding tend to relieve the arteries, as immediately as the veins, from that distension, which appears to constitute the most essential characteristic of inflammation. In a case of hemorrhage from one of the sinuses of the brain, a very judicious physician lately prescribed the digitalis: if the effect of this medicine tends principally to diminish the action of the heart, as is commonly supposed, it was more likely to be injurious than beneficial, since a venous plethora must be increased by the inactivity of the heart; but if the digitalis diminishes the general tension of the arteries, in a greater proportion than it affects the motion of the heart, it may possibly be advantageous in venous hemorrhages. We have, however, no sufficient authority for believing that it has any such effect on the arterial system in general.

Although the arguments, which I have advanced, appear to me sufficient to prove that, in the ordinary state of the circulation, the muscular powers of the arteries have very little effect in propelling the blood, yet I neither expect nor desire that the prevailing opinion should at once be universally abandoned. I wish, however, to protest once more against a hasty rejection of my theory, from a superficial consideration of cases, like that which has been related by Dr. Clarke; and to observe again, that the objections which I have adduced against the operation of the muscular powers of the arteries in the ordinary circulation, not being applicable to these cases, they are by no means weakened by any inferences which can be drawn from them.

No. XXIV.

REMARKS ON

THE EMPLOYMENT OF OBLIQUE RIDERS

AND ON OTHER

ALTERATIONS IN THE CONSTRUCTION OF SHIPS.

BEING THE SUBSTANCE OF A REPORT PRESENTED TO THE BOARD OF ADMIRALTY, WITH ADDITIONAL DEMONSTRATIONS AND ILLUSTRATIONS.*

From the Philosophical Transactions for 1814.

READ MARCH 24, 1814.

1. INTRODUCTORY OBSERVATIONS.

THE advantage derived from the employment of forces acting obliquely with respect to each other, in a variety of cases which occur in practical mechanics, has been demonstratively established by theoretical writers on the subject; and attempts have often been made to extend the application of the principle very considerably in the art of ship-building; but hitherto with very little permanent success. Mr. Seppings's arrangements are in many respects either new or newly modified; and the results of their actual employment, in the repair of the Tre-

* The Memoir which follows originated in the circumstances detailed in the following letter from Mr., afterwards Sir John Barrow, one of the secretaries to the Admiralty:—

"Admiralty Office, 19th November, 1811.

"SIR,

"Mr. Seppings, the master shipwright of Chatham Dockyard, having submitted to my Lords Commissioners of the Admiralty a model for the construction of ships of war on a new principle, by which it would appear that an advantage is obtained in point of strength and durability, while at the same time, a very considerable saving of timber is effected; and my Lords having caused a ship of seventy-four guns to be fastened according to this new mode of construction, which, after a trial of many months in the North Sea, has been found to answer every expectation that the projector himself could have formed; and being desirous of submitting this new

mendous, appear to be sufficiently encouraging to entitle them to a careful and impartial investigation, both with regard to the theory on which they are supposed to be founded, and to the facts which may be produced in their favour. The question, respecting the best disposition of the timbers of a ship, is by no means so easily discussed, as may be supposed by those, who have considered the subject but superficially; and if we allowed ourselves to be influenced by a few hasty arguments or experiments, we might be liable to the most dangerous errors: on the other hand, it may easily happen that objections to the

principle to the consideration of such men of science and practical experience as may have turned their attention to mechanics in general, and more particularly to the construction and fastening of ships:

"I am therefore directed by their Lordships to acquaint you that on Wednesday, the 27th instant, at one o'clock, Mr. Seppings will be ordered to attend at this office, for the purpose of exhibiting his model, and explaining the principle on which it is constructed; and to request the favour of your attendance on the occasion.

"I am, Sir, your very humble servant,

"*To Dr. Young.*" "JOHN BARROW."

The following is Dr. Young's reply:—

"Welbeck-street, November 22, 1811.

"DEAR SIR,

"I ought perhaps to have returned an earlier answer to your official letter, but I have made so many resolutions to forswear all further concern with the mathematical sciences, that I could not at once determine again to deviate from them by accepting their Lordships' invitation. Recollecting, however, that as far as I know, I am the only person in this country that has communicated to the public any attempts to improve the *theory* of carpentry (Lectures, Chap. XIV.), and that it would be scarcely decent to draw back on an occasion where I was called on to assist in a case of *practical* importance, I have overruled my hesitations, and shall attend on Wednesday with so much the more pleasure, as I cannot help fancying, from the little that I know of the question in agitation, I should be able, if I had leisure to discuss it thoroughly, to reconcile the discordant opinions which seem to prevail respecting it.

"I am, dear Sir,

"Your faithful and obedient servant,

"T. YOUNG."

The proposition of the important changes which Sir Robert Seppings introduced into our system of naval architecture occasioned no small amount of discussion. They were advocated by Sir John Barrow, with great vigour and ability, in several articles of the 'Quarterly Review,' and were vehemently opposed by many of the officials of the dockyards, as well as very generally by the older officers of the navy. Dr. Young's Report, and the Memoir which was founded upon it, though approving generally of the proposed changes, was too reserved in its statements to satisfy either party: whilst the mechanical and other problems which the investigation involved were of too high an order of difficulty to be easily understood by those persons for whose especial benefit it was prepared. "Though science is much respected by their Lordships," writes an official of the Admiralty, "and your paper is much esteemed by them, it is too learned."

The changes and improvements introduced by Sir R. Seppings, and their experimental results, form the subjects of several papers in the 'Philosophical Transactions."

application of those arguments or experiments, which may occur at first sight, may be capable of being removed by a more minute investigation: and the importance of the subject requires that no assistance, which can be afforded by the abstract sciences, should be withheld from the service of the public, even by those who have no professional motives for devoting themselves to it.

2. Forces acting on a Ship.

The first consideration that is necessary, for enabling us to judge of the propriety of any arrangement respecting the construction of a ship, is to determine the nature and magnitude of the forces which are to be resisted; and the second, to inquire in what manner the materials can be arranged, so as best to sustain the strains which these forces occasion. The principal forces, which act on a ship, are the weight of the whole fabric with its contents, the pressure of the water, the impulse of the wind, and the resistance of the ground or of a rock: and we must endeavour to ascertain the degree in which any of them have a tendency to bend the ship longitudinally or transversely, or to break through any part of her texture; and to inquire into those causes, which are likely to promote or to obviate the decay of the substances employed.

3. Causes of arching. Weight.

It is unnecessary to explain here the well known inequality of the distribution of the weight and pressure, which causes almost all ships to have a tendency to arch or hog, that is, to become convex upwards, in the direction of their length. It is possible that there may be cases in which a strain of a very different nature is produced: but in ships of war, this tendency appears to be universal. It is however very different in degree in the different parts of a ship; and of course, still more different according to the different modes of distribution of the ballast and stores, which may occur in different ships: but in ordinary cases, it will probably be found nearly such as is re-

presented in the calculations subjoined in the note,* deduced from data which have been obligingly furnished by an acute and experienced member of the Navy Board.

* In a modern 74-gun ship, fitted for sea, the length being 176 feet, the breadth 47½, the forces are thus distributed:—

Aftermost	49 f.	Weight 699	Pressure 627	Difference 72 tons.
Next	20	297	405	−108
	50	1216	1098	118
	20	290	409	−119
	37	498	461	37
	176	3000	3000	00

Now the laws of equilibrium will not allow us to suppose these forces concentrated in the middle of the respective portions, or equally distributed through them; and it becomes necessary, that one of the weights should be situated further forwards; which must be that of the foremost portion, containing the bowsprit and its rigging. It is also natural to suppose the excesses of weight and pressure distributed with as few abrupt changes as possible, so as to neutralise each other at the common termination of the adjoining portions, and to become more unequal in parts more remote from these neutral points. Thus the excess of weight in the first 49 feet being 72 tons, it may be supposed to begin at the rate of $\frac{144}{49}$ tons per foot, and to diminish gradually and equably, so that its centre of action will be at the distance $\frac{49}{3}$ from the end: the excess of pressure must increase in the next place, until at the distance of 59 feet from the stern, it becomes $\frac{108}{10}$ per foot, and must then diminish until it vanishes at 69, where the excess of weight must begin to prevail, becoming, at 94, $\frac{118}{25}$ per foot, and vanishing at 119. The excess of pressure might then be supposed to increase gradually through the next portion, in order to avoid an abrupt change at its extremity; but this supposition would still be insufficient, and it becomes necessary to imagine that for 6.6 feet the forces remain neutralised, and the pressure then prevails, so that its excess becomes at last $\frac{119}{6.7} = 17.7$ per foot: it must then decrease for 17.5 feet, and the excess of weight at the extremity must become 19.7 per foot, the neutral point being at 156.5. The equilibrium of the forces will then be expressed by the equation $72 \times 16.3 - 108 \times 59 + 118 \times 94 - 119 \times 134.5 - 155 \times 144.8 + 192 \times 169.5 = 0$, which is sufficiently accurate for every purpose.

From this distribution of the forces, we obtain a determination of the strain for each point of the respective portions, which is in the joint ratio of the magnitudes and distances of all the forces concerned, on either side of the point, reduced into a common result. For the first portion it is $\frac{144}{49}x \times \frac{1}{2}x - \frac{1}{6} \cdot \frac{144}{49} \cdot \frac{x}{49} \cdot x \frac{1}{5}x = \frac{72}{49}x^2 - \frac{1}{6} \cdot \frac{144}{49} \cdot \frac{x^3}{49}$, x being the distance from the stern: for the 2nd, $72(x-16\frac{1}{3}) - \frac{1}{6} \cdot \frac{108}{10} \cdot \frac{(x-49)^3}{10}$: 3rd, $72(x-16\frac{1}{3}) - 54(x-55\frac{2}{3}) - \frac{108}{20}(x-59)^2 + \frac{1}{6}, \frac{108}{100} \cdot (x-59)^3$: 4th, $72(x-16\frac{1}{3}) - 108(x-59) + \frac{1}{6} \cdot \frac{118}{25} \cdot \frac{(x-69)^3}{25}$: 5th, $72(x-16\frac{1}{3}) - 108(x-59) + 59(x-94) + \frac{118}{50}(x-94)^2 - \frac{1}{6} \cdot \frac{118}{25}$

Longitudinal Pressure.

To this strain another is added, from a cause, which, although not very inconsiderable, appears hitherto to have escaped notice;

$\frac{(x-94)^3}{25}$: 6th, from 119 to 125.6, $72\,(x-16\frac{1}{3}) - 108\,(x-59) + 118\,(x-94)$: for the 7th, we must add to this expression $-\frac{1}{6} \cdot \frac{119}{13.4} \cdot \frac{(x-125.6)^3}{13.4}$: and, in the last 37 feet, the strain will be expressed by $(176-x)\ 19.7 \times \frac{1}{2}(176-x) - \frac{1}{6}\ 19.7\ \frac{(176-x)^3}{19.5}$. Hence we find the strain, at seven points, 22 feet distant from each other and from the ends, 605, 1993, 2815, 2244, 2655, 4610, and 1875; and by taking the fluxion of x in the seventh portion, we determine the maximum at $141\frac{1}{3}$ feet, amounting to 5261 tons, supposed to act at the distance of one foot.

In order to form an idea of the curve which would be produced by such a strain, acting on a uniformly flexible substance, we may consider the curvature as represented by the second fluxion of the ordinate y, and by finding and correcting the fluent separately for each portion, we may obtain the ordinate or fall at any given point corresponding to a given extent of arching of the whole fabric. It will however be sufficiently accurate for this purpose, to consider the forces as concentrated in a limited number of points, dividing those which act in the extreme portions into two parts, in order that the curvature may be continued to the ends; so that the whole of the forces may be thus distributed at 0, 36; at $32\frac{2}{3}$, 36; at 59, -108; at 94, 118; at 134.5, $-$ 119; at 144·8, -155; at 163, 96; and at 176, 96. The strain for each portion may then be represented by $a - bx$, whence $\ddot{y} = a\dot{x}\dot{x} - bx\dot{x}\dot{x}$, $\dot{y} = ax\dot{x} - \frac{1}{2}bx^2\dot{x} + c\dot{x}$, and $y = \frac{1}{2}ax^2 - \frac{1}{6}bx^3 + cx + d$. It will be most convenient in calculation, to make x begin anew with each portion, setting out from the middle, and to divide the numbers by 100, in order to shorten the operations: thus, for the middle portion, from 88 to 59, the strain will be $.2028 + .36x$, a being .2028, and $b = -.36$; and when x becomes .22, y is .00552, and when $x = .29$, $\frac{\dot{y}}{\dot{x}} = .0740$, and $y = .0011$; which values being substituted in the equations for the next portion, we have $c = .074$, and $d = .0011$: and by going through the whole length in this manner, we find the fall at the extremes and at seven equidistant intermediate points, .08697, .05325, .02514, .00552, 0, .00507, .02531, .06705, and .12325. If we wish to find the point at which the curve is parallel to the chord of the whole, we must inquire where $c = (.12325 - .08697)$: 1.76, which will be at 98 feet, or 10 feet before the midships.

We must next determine the magnitude of the strain arising from the longitudinal pressure acting on the lower part of the ship only. The resistance being supposed to be proportional in the first instance to the degree of compression or extension, according to the common and almost necessary law of the constitution of elastic bodies, and varying also in the direct ratio of the strength of the fabric, which may be assumed to be either equable, or, in the case of a ship, proportional to the distance from a point more or less remote, we must form an equation of equilibrium for the absolute equality of the forces in opposite directions, and another for their powers of acting with respect to any given point as the fulcrum of a lever. Thus the fluxion of the absolute resistance at the distance x from the upper surface, supposing the strength to be as $a+x$, and the neutral point, at which the compression and extension cease, to be at the distance b, will be $(b-x)\ c\ (a+x)\ \dot{x} = c\ (ab - ax + bx - xx)\ \dot{x}$, which, when x is equal to the depth d, must become equal to the force f producing the strain, or $f = c\ (abx - \frac{1}{2}ax^2 + \frac{1}{2}bx^2 - \frac{1}{3}x^3)$: and for the second equation, referring the forces to the upper surface as a fulcrum, the fluent of $c\ (b-x)\ (a+x)\ x\dot{x}$, must be equal to ef, e being the distance at which the force c is applied; whence $ef = c\ (\frac{1}{2}abd^2 - \frac{1}{3}ad^3 + \frac{1}{3}bd^3 - \frac{1}{4}d^4)$. Now if we make $a = d = x$,

that is, the partial pressure of the water in a longitudinal direction, affecting the lower parts of the ship only, and tending to compress and shorten the keel, while it has no immediate action on the upper decks. The pressure, thus applied, must obviously occasion a curvature, if the angles made with the decks by the timbers are supposed to remain unaltered, while the keel is shortened, in the same manner as any soft and thick substance, pressed at one edge between the fingers, will become concave at the part compressed (Lect. Nat. Phil. I. Pl. 9. F. 117); and this strain, upon the most probable supposition respecting the comparative strength of the upper and lower parts of the ship, must amount to more than one third as much as the mean value of the former, being equivalent to the effect of a weight of about 1000 tons, acting on a lever of one foot in length, while the strain, arising from the unequal distribution of the weight and the displacement, amounts where it is greatest,

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the equations become $c\,(\frac{3}{8}bd^2 - \frac{5}{6}d^3) = f$, and $c\,(\frac{5}{8}bd^2 - \frac{7}{12}d^4) = ef$, and from the former we have $c\,(\frac{5}{8}bd^2 - \frac{25}{24}d^4) = \frac{5}{8}df$; and, by subtraction, $\frac{13}{192}cd^4 = (\frac{5}{8}d - e)f$: consequently the force f may be considered as acting on a lever of the length $c - \frac{5}{8}d$: and if we take any other value for a, the fractional multiplier of d, instead of $\frac{5}{8}$, will be $\frac{3a + 2d}{6a + 3d}$: thus if $a = \frac{1}{2}$, we have $e - \frac{7}{12}d$ for the length of the lever. In order to find the mean distance e at which the pressure of the water acts, we may suppose the form of the mean transverse section of the ship to be parabolic, and the area such as to correspond to the bulk of 3000 tons of water, each containing 35 cubic feet, the length being 176 feet, and the breadth $47\frac{1}{2}$, whence the depth must be 18.84 feet: then the centre of gravity of a parabola being at the distance of $\frac{3}{5}$ of the depth from the vertex (Vince's Fluxions, p. 101), and the centre of oscillation at $\frac{5}{7}$, when the point of suspension is at the vertex (p. 111), the distance of these points $\frac{4}{35}$ will be increased to $\frac{6}{35}$, when the point of suspension is removed to the termination of the abscis, and the distance of the centre of pressure from the vertex will be $\frac{3}{5} - \frac{6}{35} = \frac{3}{7}$, and $\frac{3}{7} \times 18.84 = 8.074$, which, subtracted from $\frac{1}{3} \times 40 = 17.777$, leaves 9.703 for the length of the lever. Now the magnitude of the pressure on this section must be to 3000 tons, as the depth of the centre of gravity, 7536 feet, to 176, that is, 128.45 tons, which, acting at the distance 9.703, will produce a strain of 1247 tons, or, in the terms of the preceding calculation, .1247, which is the multiple of $\frac{1}{2}x^2$ indicating the fall. These different causes of arching being independent of each other in their operation, their effects will be simply united into a common result: and the whole curvature of the ship, supposing its strength equable throughout its length, may be thus represented:

Distance from the stern .	0	22	44	66	88	110	132	154	176
Strain .	1247+0	605	1993	2815	2224	2655	4610	1875	0
Fall .	.04828	.02716	.01207	.00302	.00000	.00302	.01207	.02716	.04828
	.08697	.05325	.02514	.00552	.00000	.00507	.02531	.06705	.12325
	.13525	.08041	.03721	.00854	.00000	.00809	.03738	.09421	.17153
For 12 inches of arching	10.58	6.29	2.91	.67	.00	.63	2.93	7.37	13.42.

that is, about 37 feet from the head, to 5260, in a 74-gun ship of the usual dimensions; and although the strain is considerably less than this exactly in the middle, and throughout the aftermost half of the length, it is no where converted into a tendency to "sag," or to become concave. It must, however, be remembered, that when arching actually takes place from the operation of these forces, it depends upon the comparative strength of the different parts of the ship and their fastenings, whether the curvature shall vary more or less from the form, which results from the supposition of a uniform resistance throughout the length. An apparent deviation may also arise from the unequal distribution of the weight through the breadth of the ship: thus the keel may actually sag, under the step of the mainmast, even when the strain, as here calculated, indicates a contrary tendency with respect to the curvature of the whole ship.

Force of the Waves.

The magnitude of the strain on the different parts of the ship is subjected to very material alterations, when she is exposed to the forces of the wind and waves. The effect of the wind is generally compensated by a change of the situation of the actual water-line, or line of fluitation, so that its amount may be estimated from the temporary or permanent inclination of the ship; and the force of the waves may be more directly calculated from their height and breadth. These two forces can seldom be so applied, as to combine their effects in producing a strain of the same kind in their full extent; it will therefore be sufficient for our purpose to determine the probable amount of the force of the waves, which is more materially concerned in affecting the longitudinal curvature than that of the wind. As a fair specimen of the greatest strain that is likely to arise from this cause in any common circumstances, we may consider the case of a series of waves 20 feet in height, and 70 in breadth; the form being such, that the curvature of the surface may be nearly proportional to the elevation or depression: a single wave might indeed act more powerfully than a continued series, but such a wave can scarcely ever occur

singly. We shall then find upon calculation,* that the greatest strain takes place, in a 74-gun ship, at a distance of about 18 feet from the midships, amounting to about 10,000 tons at the

* The strain, produced by the pressure of waves of given magnitude, may be calculated from the comparison of the displacement with respect to their surface, compared with the displacement with respect to a level surface. It is true that the pressure upon the ship's bottom is not immediately derived from the temporary height of the nearest portion of water; but the horizontal motion of the water, which is the proximate cause of the elevation, is equally capable of affecting the fluid under a floating body, and of causing a pressure against it: the effect being nearly similar to the transmission of sound through an elastic medium. In other cases, the actual height of the fluid, over every particle concerned in the transmission of a wave, has been supposed, in calculations, to determine the pressure on it: but, whether from the necessary constitution of a fluid, or from the imperfect fluidity of fluids actually existing, it appears that there is a lateral communication of pressure within a certain angular extent, somewhat like the lateral friction attending the motions of fluids; and this is the most probable cause of the deficiency in the velocity of waves, when their breadth is very small in proportion to the depth of the fluid. In the present calculation, however, the considerations are more simple, and we have only to determine the effect of the difference produced during the passage of a wave in the quantity of water displaced by the ship, with respect to the general surface. The total height of the waves being 20 feet, and the total breadth 70, the section being supposed to constitute a figure of sines, the elevation or depression, at the distance x from the middle, will be $10 \cos. px$, p being $\frac{6.2832}{70} = .08976$, the fluxion of the area $10\dot{x} \cos. px$, and the fluent $\frac{10}{p} \sin. px$; at the constant distance z from the middle, the fluxion of the p strain will be $(x-z)\ 10\dot{x} \cos. px$, in order to find the fluent of which we must take the fluxions of $x \sin. px$, and $\cos. px$, which are $\dot{x} \sin. px + xp\dot{x} \cos. px$, and $- p\dot{x} \sin. px$; hence $\int x\dot{x} \cos. px = \frac{1}{p} x \sin. px + \frac{1}{pp} \cos. px$; and the fluent of the strain will be $\frac{10}{p}(x - z) \sin. px + \frac{10}{pp} \cos. px + c$, which must vanish when $x = z$, so that $c = -\frac{10}{pp} \cos. pz$: now, when $x = a$, the corrected fluent becomes $\frac{10}{p}(a - z) \sin. pa + \frac{10}{pp} \cos. pa - \frac{10}{pp} \cos. pz$; and if we take the fluxion of this, making z variable, we find, for the maximum, $-\frac{10}{p} \dot{z} \sin. pa + \frac{10}{p} \dot{z} \sin. pz = 0$, and $\sin. pz = \sin. pa$, so that z must be $a - 70 = 18$, whence the greatest strain is found, $\frac{10}{p} \times 70 \times .999 = 7791$, expressed in square feet of the longitudinal section, which, for a ship $47\frac{1}{2}$ feet wide, may be reduced into tons, by multiplying it by $\frac{47.5}{35}$, and will become 10572.

It is true, that if the waves allowed time for the ascent or descent of the ship, so that she might float in equilibrium, the greatest strain would be little more than $\frac{2}{3}$ of this weight; but the elevating force in the case here calculated being only $\frac{1}{10}$ of the whole weight, it would require almost a second to raise the ship 1.265 feet, and to restore the equilibrium; so that notwithstanding its gradual application, dependent on the progressive velocity of the waves, which varies with the depth of the fluid, there must be an interval during which it operates very nearly in its whole extent, especially as the occurrence of a partial obstruction tends to increase the total height of a wave at the point where it is situated.

instant when the ship is in a horizontal position, while in more common cases, when the waves are narrower, the strain will be proportionally smaller and nearer to the extremity. Hence it appears that the strain produced by the action of the waves may very considerably exceed in magnitude the more permanent forces derived from the ordinary distribution of the weight and pressure, being, according to this statement, nearly three times as great; so that when both strains co-operate, their sum may be equivalent to about 15,000 tons, acting on a lever of one foot, and their difference, in opposite circumstances, to about 5000. There may possibly be cases in which the pressure of the waves produces a still greater effect than this; it may also be observed, that the agitation accompanying it tends to make the fastenings give way much more readily, than they would do if an equal force were applied less abruptly. At the same time, it is not probable that this strain ever becomes so great, as to make the former perfectly inconsiderable in comparison with it, especially if we take into account the uninterrupted continuance of its action: it appears therefore to be highly proper that the provision made for counteracting the causes of arching should be greater than for obviating the strain in the contrary direction: for example, that if the pieces of timber intended for opposing them were, on account of the nature of their fastenings, or for any other reason, more capable of resisting compression than extension, they should be so placed as to act as shores rather than as ties: although it by no means follows, from the form which the ship assumes after once breaking, that the injury has been occasioned in the first instance by the immediate causes of arching: since, when the fastenings have been loosened by a force of any kind, the ship will naturally give way to the more permanent pressure, which continues to act on her in the state of weakness thus superinduced.

4. Breaking Transversely.

The pressure of the water against the sides of a ship has also a tendency to produce a curvature in a transverse direction, which is greatly increased by the distribution of the

weight, the parts near the sides being the heaviest, while the greatest vertical pressure of the water is in the neighbourhood of the keel. This pressure is often transmitted by the stanchions to the beams, so that they are forced upwards in the middle: when they are unsupported, the beams are more generally depressed in the middle by the weight of the load which they sustain; while the inequality of the pressure of the water co-operates with other causes in promoting the separation of the sides of the ship from the beams of the upper decks. On the other hand the weight of the mainmast often prevails partially over that of the sides; so that the keel is forced rather downwards than upwards in the immediate neighbourhood of the midships. The tendency to a transverse curvature is observable, when a ship rests on her side, in the opening of the joints of the planks aloft, and in their becoming tighter below: although this effect depends less immediately on the absolute extension and compression of the neighbouring parts than on the alteration of the curvature of the timbers in consequence of the pressure.

5. Lateral Curvature.

In such a case there is also an obvious strain tending to produce a lateral curvature; and shores are sometimes employed to prevent its effects, when a ship is "hove down" on her side. This indeed is comparatively a rare occurrence; but when a series of large waves strikes a ship obliquely, they must often act in a similar manner with immense force: the elevation on one side may be precisely opposite to the depression on the other; and the strain from this cause can scarcely be less than the vertical strain already calculated: but its effects are less commonly observed, because we have not the same means of ascertaining the weakness which results from it, by the operation of a permanent cause. When a ship possesses a certain degree of flexibility, she may in some measure elude the violence of this force by giving way a little for the short interval occupied by the passage of the wave; but it may be suspected that her sailing, in a rough sea, must be impaired by such a temporary change of form.

6. Grounding.

When a ship takes the ground, she may either give way at once to the stroke of a rock, or rest on a bottom more or less soft, until she is either wholly or partially abandoned by the water. In the former case her resistance must depend in great measure on the parts in the immediate neighbourhood of the injury: in the latter it may happen, that she may be supported by so large a surface, as to be more in danger of parting aloft than of being crippled below. Commonly, however, the floor timbers are forced in at one end, the first futtocks, which are their immediate continuations, being broken off; and sometimes the opposite ends of the floor timbers are forced out, especially in large ships without riders, their attachment to the keel remaining unimpaired.

7. Decay.

The causes which promote the decay of timber are only so far understood, as we are acquainted by experience with their effects. A partial exposure to moisture appears to be by far the most general of these causes: it is well known that total submersion does not accelerate decay; a surface which is kept moist by imperfect contact with another, so that a portion of water is retained between them by capillary attraction, seems always to be the part at which the timbers begin to rot: while both the surfaces completely exposed either to the drier air, or to the water, and those which are wedged closely together, and press strongly against each other, remain perfectly sound.

8. Means of Resistance.

We are next to inquire into the comparative advantages of different angular positions of the timbers of a ship for resisting the forces which have been described; and in particular how far the arrangements, which have been proposed by Mr. Seppings, are better calculated for the purpose, than the common modes of construction. Whatever opinion we may ultimately form of these arrangements, they are by no means sufficiently

justified by the experiments which have been exhibited in illustration of them. These experiments show, that when two parallel planks (fig. 132) have loose pieces interposed, extending perpendicularly from one to the other, they are incomparably weaker, with respect to any transverse force, than when the intermediate pieces are in an oblique direction, so as to constitute a frame, which can only be bent as a whole. But it cannot for a moment be imagined, that the planks of a ship are connected with the timbers in as loose a manner as these transverse braces, which will scarcely support their own weight for the purpose of the experiment; and in fact the comparison would have required, that the whole space included by the parallelogram should be filled up in each case by similar braces, or at least that the two planks should have been firmly united at the loose end to the transverse braces (fig. 134); and it is demonstrable that in this case the same weight would have broken the pins, as if one of the planks had been oblique, or as if the planks had remained parallel, and had been connected by oblique pieces.

Such a result would, however, be far from proving the inutility of the addition of oblique braces to a rectangular frame: for the kind of strength required for any particular purpose is not always determined by the magnitude of the force which would be capable of breaking the substances concerned, although the power of resisting such a force is properly called strength, in the most limited sense of the term: but there are many occasions on which stiffness or inflexibility is of still greater consequence than strength, and others again on which flexibility is of material advantage. A coach spring, consisting of ten equal plates, would be rendered ten times as strong, if it were united into one mass, and at the same time a hundred times as stiff, bending only one hundredth of an inch with the same weight that would bend it a whole inch in its usual state, although nothing would be gained by the union with respect to the power of resisting a very rapid motion, which I have, on another occasion, ventured to call resilience. (Lect. Nat. Phil. I. p. 143. II. p. 50.) Now it appears to be extremely difficult to unite a number of parallel planks so firmly together,

by pieces crossing them at right angles, as completely to prevent their sliding in any degree over each other: and a diagonal brace of sufficient strength, even if it did not enable the planks to bear a greater strain without giving way, might still be of advantage, in many cases, by diminishing the degree in which the whole structure would bend before it broke.

The strength of a simple rectangular frame, firmly fixed at one end, is rendered somewhat less than double by perfectly fastening the joints at the other (fig. 135,) and the stiffness is nearly quadruple.*

The comparative security, obtained by the addition of a diagonal brace, is almost without limit. Supposing any number of planks of equal dimensions to lie simply on each other without any adhesion, and to be firmly fixed at one end, their aggregate strength will be very little greater than that of a single plank of one sixth part of the common depth or thickness of each, supported by a brace a little stronger, in the direction of the diagonal of the whole, (fig. 136;) and the stiffness of the parallel planks will be as many times less than that of such a frame, as there are planks in one third of the whole series. Thus if we

* When two horizontal bars are firmly fixed at one end only, and simply united at the other end by a vertical piece, their immediate joint force in resisting flexure remains unaltered; but if the vertical piece is firmly fixed to the ends of the bars, it may be considered as a lever held in equilibrium by four forces, arising from the repulsive and cohesive powers of the separate bars; and the sum of these forces must vanish when reduced to the same direction, while the sum of their actions, referred to any point as a fulcrum, vanishes also: and it is obvious that the total compression of the one bar will initially be equal to the total extension of the other, provided that their strength be equal. Hence, if the mean distance of the bars be a, and the depth b, reckoned between the centres of action of the respective forces, which in perfectly elastic bodies will be $\frac{2}{3}$ of the whole depth, the first force being x, and the second $-y$, the third will be y, and the fourth $-x$, and, from the equilibrium with respect to the point of application of the first force as a fulcrum, we have the equation $-by + ay - (a + b)x = 0$, and $x = \frac{a - b}{a + b} y$, while the joint effect of all the forces in resisting the pressure of the weight is $2(y + x)b : c$, c being the length of the bars, or $\frac{2by}{c} \cdot \frac{2a}{a + b}$, while the resistance of the two single bars would be $\frac{2by}{c}$, the inclination of the elementary forces being here reduced to $\frac{b}{2c}$: and since the magnitude of y at the instant of breaking is given, the force will be augmented by the firmness of the connexion in the ratio of $2a$ to $a + b$, which is always less than that of 2 to 1. The stiffness may be nearly quadrupled by the fastenings, since the depression at the moment of breaking is reduced to little more than one half.

had twelve planks, six inches deep, and one thick, with friction rollers interposed, it is demonstrably true, however surprising, that they would be very little stronger in supporting a weight at the end, than a single tie an inch square in its section, assisted by a diagonal brace of equal relative strength: and also that this apparently slight structure would be nearly four times as stiff as the 12 planks, being depressed only one fourth as much, with a given weight, as the planks with a similar force acting on them.*

It is well known, that if the planks were firmly united into one mass, their strength would be rendered 12 times as great by the union, and their stiffness 144 times: but this is not the greatest resistance of which the materials are capable, even without any extension of their base of support: for if the planks were connected in pairs at half the distance of the whole depth, and allowed to move freely round fastenings perfectly secure, their strength, speaking theoretically, would be greater by nearly one half than if they formed a compact mass, while their stiffness would be only about one fourth as great: and an effect nearly similar might be produced if the respective pairs were united by oblique braces, extending over half the depth of the whole structure, although it would be very difficult, in practice, to make the strength of an arrangement

* "If one of the surfaces of a beam were incompressible, and the cohesive force of all its strata collected in the other, its strength would be six times as great as in the natural state." Lect. Nat. Ph. II. 50, Art. 335.—Hence a plank of $\frac{1}{6}$ of the actual depth, acting simply as a tie, supported by a brace fixed at the distance of the depth, would be as strong as the original plank: and by increasing the distance of the point of support of the brace in the ratio of the number of planks, the strength of the two arrangements will remain equal, without altering the dimensions of the tie. The length of a plank being e (Lect. II. 48, Art. 326), the depth b, the height of the modulus of elasticity m, the depression d, and the force applied at the end equal to the weight of a similar plank of the length g, we have $m = \frac{4e^3}{bbd}g$, and $d = \frac{4e^3}{bbm}g$; but, for a simple frame of two equal pieces, the force being g, the longitudinal extending force will be the weight of $\frac{e}{b}g$, and the actual extension $\frac{e^2}{b} \cdot \frac{g}{m}$, and the depression $\frac{2e^3}{bbm}g$, half as great as that of a plank of the same dimensions, when g is given, or supposing the weight on the frame sextuple, so as to be equal to that supported by the plank of six times the depth, three times as great; but, by taking 12 planks together, we increase their stiffness only 12 times, while that of the frame is rendered 144 times as great by a similar extension of the base, so that it becomes in this case 4 times as great as that of the 12 planks.

of this kind even equal to that of a compact mass, since the fastenings could never be so perfect, as to bring every fibre of each plank into its full action at once, as the theory supposes. If the planks were already united into a compact mass, so as to be incapable of bending except as a whole, it is of importance to inquire whether any advantage would be gained by the further addition of oblique braces: and it will appear that if the braces were fixed to the outermost planks of the series only, they would have no manner of effect either on the strength or on the stiffness, whatever might be their direction; but if they were sufficiently fastened throughout their extent to each plank with which they come into contact, they would add both to the strength and to the stiffness, very nearly in the same degree as if they were fixed in the direction of the planks, at a distance from each other equal to their shortest actual distance, so as to constitute as many ribs as there are braces in a transverse line (fig. 137).* Hence, although there is obviously no economy in such an employment of oblique braces, yet it is by no means true that oblique braces are incapable of adding to the strength of a structure composed of pieces arranged at right angles; the assertion might however be very nearly correct in circumstances approaching to those of one of the experiments which have been exhibited for the purpose of illustrating the utility of such braces. On the other hand, the advantage of employing oblique braces must depend in great measure on the degree in which the angular position of the structure would be susceptible of variation without them; since, when properly fastened, they must universally tend to preserve the form unaltered, although they are somewhat less calculated to add to the ultimate strength of the principal tie or shore, than if their direction had been longitudinal. To take, for example, the case

* The extension and compression of the whole fabric being supposed equal, the diagonal braces will undergo no change of length, and therefore will not assist in the resistance, if only attached at their extremities. But in reality, although the extension and compression may be very nearly equal in the first instant of the change of form, the extension will always be much greater after a certain time, from the imperfection of the fastenings, which will allow the parts to separate while their own resistance prevents their compression in a material degree: so that oblique braces, however fixed, must in this respect add considerably to the strength.

of a ship's arching or hogging: if the strength were overcome without any deficiency of stiffness, the upper decks and wales would be elongated, and the butts of the planks aloft parted, while the keel would be somewhat shortened, and the planks near it crippled, so that a ship 176 feet long and 40 feet deep, arching one foot with a uniform curvature, would have the length of the parts aloft, on the level of the quarter deck, 22 inches greater than that of the keel. If, on the contrary, the strength were not overcome, but the stiffness only failed, the angular situation of the parts being altered, and the joints simply becoming loose without parting, the planks would slide on each other, and their square ends would no longer remain in the same vertical line at the ports, while there would be no material alteration in the comparative length of the decks and keel, nor any permanent parting of the butts of the planks.

Grounds of Decision respecting Oblique Riders.

This comparison therefore brings the question, respecting the general utility of oblique riders, into a very narrow compass; and we have only to inquire in what way it is most usual for ships to exhibit symptoms of weakness, in order to decide it. Now it will appear that, in cases of arching in general, some of the butts of the planks are always found to have parted aloft, at the same time that the angular position of some parts of the structure has as uniformly been more or less altered; and very generally a certain degree of sliding is observable in the planks at the sides of some of the ports. This sliding is seen very distinctly in the planks of the Albion and of the Belliqueux, now at Chatham: at the same time there are also obvious indications of a certain degree of extension and compression: in the Albion, the butts of the planks have parted so far, that in some instances pieces have been let in between them; and in the Belliqueux, there is a space of about five inches between the middle of the deck transom and the carling which had originally been in contact with it. In the Asia, lately launched in the Medway, the arching amounted to three inches and a quarter, and the comparative length of the upper and lower parts was probably altered about two

inches at most: the parting of the butts amounted to $\frac{3}{16}$ of an inch each " for upwards of fifty feet in length in the midships, and for about eight feet from the top of the side," making a total extension of probably less than an inch: so that about half the effect seems to have been produced in one way, and half in the other: but apparently the greater half by the want of stiffness. It is also usually observable, that there has been some degree of permanent compression or crippling below, the butts of the planks opening when the cause of arching has been removed, and the sheathing being more wrinkled than would have happened from the simple bending of the planks. Where it has been observed, that the fore part of all the treenails supported the pressure of the planks in the after part of the ship, and the after part in the fore part of the ship, the observation must probably have been made on the lower parts of the ship, from the effect of a partial compression of this kind.

Authorities.

From this statement it appears that unless some very strong facts can be produced, to disprove the probability, that the relative angular position of the parts constituting a ship may always be materially altered, without an absolute failure of strength, it cannot be denied that the principle of oblique bracing offers a remedy for the tendency to arch, whatever doubts there may be of the efficacy of any particular mode of applying it. And even if no observations could be produced in confirmation of the frequent occurrence of such a change of the angular situation of the timbers, the supposition that the stiffness could be perfect in this respect, notwithstanding the unequal shrinking of the timbers, and other similar circumstances, while the ultimate strength gave way by the failure of the fastenings, is in itself so highly improbable, that no positive evidence would be required for its complete rejection. We shall find, accordingly, that Mr. Bouguer takes for granted the existence of a partial flexure, as sufficiently admissible without direct proof, and recommends the adoption of oblique planking as a remedy; and that other experienced authors have been equally favour-

able to the employment of some similar arrangements.* In speaking of Mr. Gobert's mode of placing the ceiling of a ship obliquely, Mr. Bouguer observes, that "this method cannot fail of producing the most desirable effects; for when the planking both within and without was arranged in the direction of the keel, it happened, in case of the ship's arching, that the rectangles formed by the timbers and the planking, merely changed their figure a little, so as to become rhomboids, two of the angles opening a little, while the other two became more acute: but when the planks of the ceiling are laid in an oblique direction, they serve as diagonals to the rectangles, so that a simple change of the relative angular situations of the sides is not sufficient to admit of the arching, without an alteration of the length of the diagonals, which would afford a resistance incomparably greater, especially at the upright parts of the sides, although at the floors it would have but little effect." Traité du Navire, 154. Mr. Groignard also, whose memoir, on the improvement of ship-building, has been obligingly communicated to me by an ingenious gentleman, formerly his pupil, although he objects to Mr. Gobert's method, confesses that he "should have very much approved this mode of disposing the ceiling, if it had been possible to employ straight planks, having the same obliquity without interruption, throughout the whole of the ship's length;" but thinks, with Bouguer, that in carpentry, "every interruption is to be avoided as dangerous;" an objection so vague, as neither to require nor to admit a very distinct reply. Don George Juan, too, after a calculation of the absolute strength of the pieces of timber employed in the construction of a ship, very properly remarks, that the effect of arching must be attributed not to their want of strength, but to "their play on each other."

9. Mr. Seppings's Braces.

It appears, therefore, to be sufficiently established, that the principle of employing oblique timbers is a good one, provided that it be so applied as to produce no practical inconvenience.

* See Sir John Barrow's observations on this statement. Quarterly Review, vol. xii., p. 457—*Note by the Editor.*

We must next inquire whether Mr. Seppings has introduced it in a manner likely to be effectual, and not liable to any material objections. He places, on the sides of a 74-gun ship, several series of oblique braces, principally between the ports, in the place of the internal planking, making an angle of about 24° with the decks; consisting of planks 4 inches thick, and about 11 wide, coaked and bolted to the timbers, and abutting against upright pieces similarly fastened. Now it follows, from what has already been stated, that these pieces have about four-fifths as much effect in co-operating with the neighbouring parts, which act horizontally, as if they had been placed in the same situation with them, even on the supposition that the relative angular situation of the pieces is unalterably fixed; but for preventing the alteration of this situation, there is no doubt of their being very advantageously arranged, so far as their strength is sufficient; and the existence of a tendency to such an alteration, in a very material degree, appears to be altogether indisputable. Below the gun deck, the oblique timbers are considerably stronger, although they act under circumstances somewhat less favourable.

If, however, the resistance of a part of a structure is very immediately directed against a certain force, without an adequate co-operation from other parts of that structure, and if being abandoned by those parts, it is exposed to a strain which it is too weak to withstand, it is obvious that it must inevitably be the first to give way, and must leave the rest of the fabric more exposed to be overpowered by such a force, than before its introduction. We must therefore inquire, how far it is possible that Mr. Seppings's braces should be so abandoned. Now supposing a 74-gun ship to arch two feet, and one half of the change to depend on the sliding of the planks over each other, which will be allowed, by those who doubt the utility of the arrangement, to be fully as much as can ever happen; the greatest fall of the surface will be one foot in 44, and the length of the brace will be diminished $\frac{1}{176}$ or $\frac{7}{176}$ of an inch in the length of six feet, which, with a moderate allowance for the partial yielding of the fastenings, it will be perfectly capable of supporting without being crippled, although indeed it could

scarcely support much more. It is obvious, however, that this supposition in many respects far exceeds the utmost that can possibly happen: and it would even require a greater force to produce such an effect on the braces, than any which the ship actually sustains. In order to calculate the magnitude of the greatest strain which these pieces could support, it will be safest to proceed on the supposition, that each square inch of the section of good oak timber is capable of resisting the pressure of four tons on an average: it will then appear that a single series of such braces, as Mr. Seppings employs, extending throughout the length of each side of the ship, would support a weight of 143 tons, in whatever way the force counteracting it might be applied; and estimating the effect of all the braces and riders as equivalent to about four such series, the whole would resist a force of 570 tons; while the greatest force derived from the distribution of the weight together with the action of such waves as we have considered, amounts to about 450 tons: so that the strength of these braces can scarcely be insufficient to support the pressure, unless the ship should be left dry, resting on the middle of her keel, and the braces should be abandoned by all the other parts which usually co-operate with them.* The fastenings must indeed be considerably weaker than this, and the other parts of the ship considerably stronger; but since the fastenings appear to possess sufficient strength to resist any strain which is actually likely to affect them, there seems to be no inconvenience in their inferiority to the other parts. In fact, the Tremendous actually supported, for three days, without any perceptible change of form, a strain

* If a jointed parallelogram, composed of pieces of invariable length, having one of its sides fixed in a vertical position, be supported by a diagonal brace, the compression or extension of the brace will be to the descent of a weight connected with the moveable end of the parallelogram, as the depth of the parallelogram to the length of the brace, whatever the actual distance of the weight may be; so that although the strain on the horizontal pieces increases with this distance, that which affects the brace is independent of it; the relative being to the absolute strength as the depth of the frame is to the length of the brace. We must therefore inquire, what is the greatest absolute force that can be supposed to urge a given portion of the fabric in either direction: thus the excess of weight which has been attributed to the bowsprit and the neighbouring parts being 192 tons at 19½ feet from the head, this force may be occasionally increased by a similar pressure derived from the effect of the waves, which alone would amount to 302 tons at 35½ feet from the head, and which may sometimes co-operate with the former, so as to constitute a force of about 450 tons, about 25 feet from the head.

fully equal to that which is here calculated, having been purposely left on shores, which extended through 52 feet only of her length. But it must be remembered, that such a force, from its very gradual application, must be much less trying to the ship's strength, than the more abrupt changes which occur at sea, and it must on the whole be inferred, that it would be unsafe to trust to the braces alone, unsupported by the co-operation of the neighbouring parts. It would probably be easy to add some further strength to these braces near the ends of the ship, where the strain on them is the greatest, especially about 30 feet from the head, if it were found that they gave way before the rest of the timbers; and it might also be possible to replace them, if they had once failed, with greater ease than many other parts of the fabric.

It may be questioned how far it is allowable to omit any part of the inner planks between the ports, for which the braces are a substitute, on account of their utility in securing the butts of the planks, which are always made to shift where they are supported by this subsidiary tie: but with the outer planking which remains, and with the partial assistance of the braces, to say nothing of that of the shelf pieces, it can hardly be believed, that the tie is more likely to part between two ports of the same deck, than immediately over one of them.

It has been very ingeniously observed, that arching is not only a part of the evil occasioned by a ship's weakness, but that it has an immediate tendency to afford a partial remedy for the cause which produces it, by making the displacement greater at the extremities of the vessel, and smaller in the middle; but, in fact, this change appears to be too inconsiderable in its extent, to produce any material benefit: the strain at the midships being diminished by each inch of arching only 66 tons, supposed to act at one foot: so that very little relief is obtained from the change, in comparison with the whole strain.

The case of the Kent, which broke in a remarkable degree, notwithstanding the employment of riders of large dimensions, is perfectly reconcileable with the principles which have been laid down: indeed these riders, which made an angle of a few

degrees only with a vertical line, could have so little effect either on the strength or on the stiffness of the structure, that there was not the slightest reason to expect any material advantage from their application.

The explanation which has been given of the universal tendency of ships of war, in all common circumstances, to arch throughout their length, is sufficient to justify the different directions in which Mr. Seppings now arranges his braces in the different parts of the ship, since they must necessarily afford a greater strength as shores than as ties, and since the most permanent and the greatest strain will generally be such as to call them into action in this capacity. When, however, a ship is compared to an inverted bridge, it must not be forgotten how necessary it frequently becomes, to consider these braces in a different capacity, and to provide for this contingency, as indeed Mr. Seppings has not neglected to do, by employing such fastenings, as are extremely well adapted to secure their action as ties.

The shelf pieces, which Mr. Seppings employs, and the superior strength of the fastenings of his decks to the breast hooks and transoms, have so obvious a tendency to counteract the causes of arching, that it is unnecessary to insist on their utility; the weight and expense of the shelf pieces are probably the only drawbacks upon the advantages, which they are so manifestly calculated to afford, in resisting both a vertical and a lateral strain; and even in these respects, they appear to be preferable to the wooden knees formerly employed.

The filling up the intervals of the timbers, throughout the hold, with wedges of old stuff, is perhaps the most indisputably beneficial of all the alterations which Mr. Seppings has either introduced, or revived in an improved form. The strength, which is thus obtained, acts immediately in the prevention of arching, and is probably, in this respect, more than an equivalent to that of the internal planking, which has been omitted; while the cohesive strength of the external planking, considered as a tie, is still probably more than sufficient for resisting the smaller force, which occasionally operates in a contrary direction: although the strength of the ship, for resisting such a force, is

certainly much diminished by the change. From the manner in which these wedges are driven by Mr. Seppings, it may easily be understood, that they may tend to produce a convexity below, without raising any part of the keel from the blocks, merely causing it to press more strongly on them at the midships; so that if this difference becomes equal to that of the weight and pressure after launching or floating, there may be no tendency to any further change whatever; and hence it may happen, that without any other superiority of stiffness, or even of workmanship, a ship may appear wholly exempt from arching, as the Tremendous did, and some other ships are said to have done. Without the operation of some such cause, even a hollow cylinder of compact oak, 180 feet long, 50 feet in diameter, and 6 inches in thickness, if such a mass could be supposed to exist, would exhibit, when immersed to the depth of its axis, a degree of arching just perceptible, from the longitudinal pressure of the water only, amounting to about $\frac{1}{18}$ of an inch;* besides a curvature proportionally greater from the other strains, which have been already calculated. Mr. Seppings has also very properly introduced, in the Tremendous, an additional kelson on each side of the step of the mainmast, in order to support its weight, and to prevent the partial sagging of the keel.

* "The stiffness of a cylinder is to that of the circumscribing prism, as three times the bulk of the cylinder is to four times that of the prism" (Lect. Nat. Phil. II. 83, Art. 339. B.): but the radius of curvature of a prismatic beam is $\frac{bbm}{12af}$ (P. 46. Art. 321.), b being the depth, m the weight of the modulus, f the force, and a the distance of its application: and taking m for the weight of the modulus of the cylinder, its radius of curvature will be $\frac{bbm}{16af}$. But since the stiffness is as the fourth power of the diameter (P. 49, Art. 333), that of the hollow cylinder in question will be reduced in the ratio of 1 to $1 - .98^4 = .0786$. Now when a cylinder is immersed to the depth of its axis, the calculation of the effect of the longitudinal pressure exactly resembles that of the stiffness, the strain being to that which would be the effect of the pressure on the ends of the circumscribing prism as $\frac{3}{4} \times .7854 = .58905$ to 1: but the strain on the prism would be $= 50 \times 25 \times 12.5 \times \frac{50}{3} : 35 = 7440.5$, and for the cylinder, $af = 4383$: and since the height of the modulus of elasticity of oak is 5060000 feet (p. 509), and its specific gravity nearly equal to that of water, or perhaps a little greater, we have $m = 5060000 \times 50 \times 50 \times .7854 : 35$ tons, and the radius of curvature $.0786 \frac{bbm}{16af} = .0786 \cdot \frac{50^4 \times 5060000 \times .7854 : 35}{16 \times \frac{3}{4} \times .7854 \times 50^4 : (24 \times 35)} = .0786 \cdot \frac{5060000}{12 : 24} = 795432$ feet; and dividing the square of 90 by twice this number, we have .0051, or one sixteenth of an inch, for the versed sine or arching.

10. Riders.

With respect to the transverse strain, or the tendency of the sides to sink in comparison with the keel, some strength is probably gained by Mr. Seppings's mode of fixing the filling timbers in the same manner as the frames: and some advantage must be attributed to the co-operation of the wedges, or fillings in, with the timbers, as far as their connexion is capable of bringing them into action. The common ceiling is by no means advantageously placed for assisting in a resistance of this kind, since it can only act where the curvature would be increased by the bending of the sides, and even there can only be compressed in a transverse direction (fig. 138). The riders, commonly placed upon it, on the contrary, are very favourably situated for assisting in this action; but Mr. Seppings's riders are so much more numerous, as to possess, notwithstanding their obliquity, a still greater force. The fastenings of the beams to the sides are also concerned in resisting a strain of this kind, as well as in counteracting the tendency to an extension aloft, which is the more immediate consequence of the unequal pressure of the water against the ship's sides. Mr. Seppings's fastenings, so far as they depend on the shelf pieces, have probably some advantage over the more common ones; but the iron knees which he employs (fig. 139) do not appear to be quite so economically arranged as the straps of a simpler form, which other builders have used; they afford, indeed, a very direct connexion with the timbers, and they save some valuable wood in the chocks which support them: but still there appears to be some waste of strength when they act as ties, from the great obliquity of the shoulders, with respect to the direction of the force; to say nothing of the expense of the workmanship: and if, as Captain Campbell seems to have suspected, there is any slight deficiency in the transverse strength of the Tremendous at the waterways, the circumstance may afford a further reason for doubting of the utility of these fastenings.

11. Decks.

The least obvious advantage attributable to the obliquity introduced by Mr. Seppings appears to be in his mode of laying the planks of the decks; parts which seem to be principally required to co-operate with the sides of the ship as ties in a longitudinal direction: for the slight curvature, which is given to them, can no more render them incapable of such an action, than the bending of a towing rope prevents its pulling along a boat. But in the first place, the lower decks can have little or no action of this kind, from their near approach to the line, at which extension ceases, and compression begins, at least until some of the fastenings give way: and secondly, the upper decks lose but one third of their strength in this capacity by having their planks disposed at an angle of 45 degrees with the sides, while the obliquity must be capable of affording some additional power of resisting the violent action of the waves, which sometimes produces an immense strain in a transverse or lateral direction, as well as of enabling the ship, in case of necessity, to be more safely "hove down" on her side. There seems also to be some convenience in having the ends of the planks covered by the waterways, with respect to keeping the wings of the ship dry, although it has been suspected that the ends so covered may be rendered somewhat more liable to decay. It may, however, be apprehended, that any force, tending to shorten the deck, will have some little effect in forcing out the sides; for instance, if the whole deck became three inches shorter, the length of the planks remaining the same, they must force out each of the sides about a quarter of an inch, provided that their connexion with the beams allowed such a change, which appears indeed somewhat improbable. There may possibly be a slight difficulty in adjusting the planks to the curvature of the beams; but this difficulty appears to be readily overcome in other cases, as in that of the common ceiling. It may hereafter deserve to be inquired, how far an oblique direction of the carlings between the beams, which in their present situation seem to contribute very little to the strength,

might enable them to co-operate in resisting a lateral force, if the arrangement could be made without too much weakening the beams, in procuring proper abutments for these pieces.

12. Floors.

It cannot easily be admitted, that Mr. Seppings's construction affords any additional strength to a ship's bottom in case of her grounding. The fillings in between the timbers must indeed be extremely useful in this respect, first by giving firmness in the direction of the length, since even a straight plank is strengthened by having the incompressibility of its outside increased, much more one that is curved, in however slight a degree; and secondly, by co-operating with the timbers, considered as shores, so far as the wedges are fixed in their places by their lateral adhesion or otherwise.

The ceiling, which has been omitted, can have very little effect by its own strength in preventing the separation of the timbers at the floor heads; but where there are transverse riders, it must be of essential advantage in enabling these to come into action, for the support of the neighbouring parts exposed to pressure; somewhat more effectually indeed, in many cases, than Mr. Seppings's diagonal riders and their trusses can do, notwithstanding the superiority of their number and aggregate strength; on account of the magnitude of the intervening spaces, which might happen to receive the principal stroke near their centres. This magnitude does not, however, contribute by any means in the same proportion to the weakness of the parts, as it would do if the surface were plane: and it is not improbable, that for supporting the weight of the ship on a very soft ground, Mr. Seppings's arrangement might afford equal strength with the common form, as seems to have been exemplified by his experiment of leaving the Tremendous for three days on fourteen shores only, without injury: but for encountering the stroke of a rock, or of very hard ground, Mr. Seppings's ship would probably be inferior, since in this case greater stiffness, even with equal strength, would be detrimental rather than beneficial; while, on the other hand, she would un-

deniably be less liable to suffer from any injury that might happen to her outer planking only; and, from her superiority in this respect, might possibly sustain, without inconvenience, a stroke, which would be ultimately fatal to a ship of a different construction.

13. Durability.

There does not seem to be the slightest ground for the apprehension, that the filling in should render the ship's timbers liable to decay: on the contrary, the timbers of the Sandwich were found perfectly sound in the lower half of their length, opposite to the wedges which had been driven in between them, and completely decayed in the upper half, where they had been exposed, in the usual manner, to the action of the confined moist air and water; and this result is perfectly conformable to analogy with the few facts that have been ascertained, respecting the general causes of decay. The utility of the filling in, for preventing the accumulation of filth, and for keeping the ship free from foul air, with respect to the comfort, and perhaps to the health of the crew, is too obvious to require discussion. How far the economy of timber may in all cases be so great as Mr. Seppings is disposed to believe, can best be ascertained by those who are in the habit of estimating its value: but if the durability of the vessel only were improved, at an equal expense, the adoption of his alterations would still be highly advisable.

14. Conclusion.

It is by no means impossible, that experience may suggest some better substantiated objections to these innovations, than have hitherto occurred: but none of those objections which have yet been advanced, appear to be sufficiently valid to warrant a discontinuance of the cautious and experimental introduction of Mr. Seppings's arrangements, which has been commenced by orders of the Board of Admiralty. The filling in seems to be wholly unexceptionable; the braces between the ports appear to be decidedly more beneficial than the planks for which they are

substituted; and the coakings seem to be very judiciously employed in various parts of the structure: but something more may possibly be hereafter effected for the further improvement of the decks, and for the more complete provision of a substitute for the thick stuff of the ceiling, in addition to the diagonal riders, if experience should prove that there is any deficiency in the resistance of these parts. But it must be remembered, in forming conclusions from such experience, that when an arrangement of any kind has nearly attained the maximum of its perfection, it may demonstrably be varied in a considerable degree, without a proportional alteration of its effect; so that the most correct knowledge of scientific principles, and the minutest accuracy in their application, must become indispensably necessary, in order to secure us from the introduction of material errors, derived from the latent operation of accidental causes, foreign to the immediate subjects of investigation.

Welbeck Street, 30 Dec., 1811.

Fig. 132.

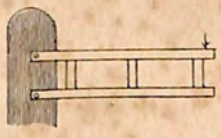

Fig. 133.

Fig. 134.

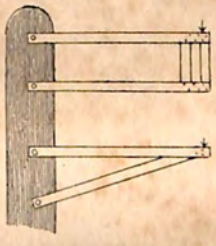

Fig. 135.

Fig. 136

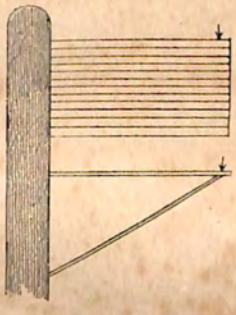

Fig. 137

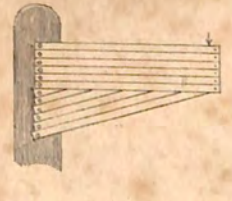

Fig. 138.

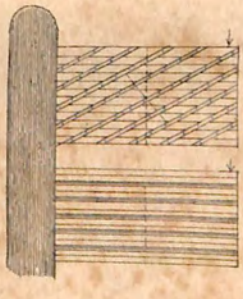

Fig. 139.

Fig. 140.

Standidge & Co. Litho. London.

No. XXV.

A NUMERICAL TABLE OF

ELECTIVE ATTRACTIONS,

WITH REMARKS ON THE SEQUENCES OF DOUBLE DECOMPOSITIONS.

From the Philosophical Transactions for 1809.

READ FEBRUARY 9, 1809.

ATTEMPTS have been made, by several chemists, to obtain a series of numbers capable of representing the mutual attractive forces of the component parts of different salts; but these attempts have hitherto been confined within narrow limits, and have indeed been so hastily abandoned, that some very important consequences, which necessarily follow from the general principle of a numerical representation, appear to have been entirely overlooked. It is not impossible that there may be some cases in which the presence of a fourth substance, besides the two ingredients of the salt, and the medium in which they are dissolved, may influence the precise force of their mutual attraction, either by affecting the solubility of the salt or by some other unknown means, so that the number naturally appropriate to the combination may no longer correspond to its affections; but there is reason to think that such cases are rare, and when they occur they may easily be noticed as exceptions to the general rules. It appears, therefore, that nearly all the phenomena of the mutual actions of a hundred different salts may be correctly represented by a hundred numbers, while, in the usual manner of relating every case as a different experiment, above two thousand separate articles would be required.

Having been engaged in the collection of a few of the prin-

cipal facts relating to chemistry and pharmacy, I was induced to attempt the investigation of a series of these numbers; and I have succeeded, not without some difficulty, in obtaining such as appear to agree sufficiently well with all the cases of double decompositions which are fully established, the exceptions not exceeding twenty out of about twelve hundred cases enumerated by Fourcroy. The same numbers agree in general with the order of simple elective attractions as usually laid down by chemical authors; but it was of so much less importance to accommodate them to these that I have not been very solicitous to avoid a few inconsistencies in this respect, especially as many of the bases of the calculation remain uncertain, and as the common tables of simple elective attractions are certainly imperfect, if they are considered as indicating the order of the independent attractive forces of the substances concerned. Although it cannot be expected that these numbers should be accurate measures of the forces which they represent, yet they may be supposed to be tolerable approximations to such measures; at least if any two of them are nearly in the true proportion, it is probable that the rest cannot deviate very far from it: thus, if the attractive force of the phosphoric acid for potash is about eight-tenths of that of the sulfuric acid for barita, that of the phosphoric acid for barita must be about nine-tenths as great; but they are calculated only to agree with a certain number of phenomena, and will probably require many alterations as well as additions when all other similar phenomena shall have been accurately investigated.

There is, however, a method of representing the facts, which have served as the bases of the determination, independently of any hypothesis, and without being liable to the contingent necessity of any future alteration, in order to make room for the introduction of the affections of other substances; and this method enables us also to compare, upon general principles, a multitude of scattered phenomena, and to reject many which have been mentioned as probable, though doubtful, with the omission of a very few only which have been stated as ascertained. This arrangement simply depends on the supposition

that the attractive force which tends to unite any two substances may always be represented by a certain constant quantity.

From this principle it may be inferred, in the first place, that there must be a sequence in the simple elective attractions. For example, there must be an error in the common tables of elective attractions, in which magnesia stands above ammonia under the sulfuric acid, and below it under the phosphoric, and the phosphoric acid stands above the sulfuric under magnesia, and below it under ammonia, since such an arrangement implies that the order of the attractive forces is this: phosphate of magnesia, sulfate of magnesia, sulfate of ammonia, phosphate of ammonia, and again phosphate of magnesia; which forms a circle, and not a sequence. We must therefore either place magnesia above ammonia under the phosphoric acid, or the phosphoric acid below the sulfuric under magnesia; or we must abandon the principle of a numerical representation in this particular case.

In the second place there must be an agreement between the simple and double elective attractions. Thus, if the fluoric acid stands above the nitric under barita, and below it under lime, the fluate of barita cannot decompose the nitrate of lime, since the previous attractions of these two salts are respectively greater than the divellent attractions of the nitrate of barita and the fluate of lime. Probably, therefore, we ought to place the fluoric acid below the nitric under barita; and we may suppose that, when the fluoric acid has appeared to form a precipitate with the nitrate of barita, there has been some fallacy in the experiment.

The third proposition is somewhat less obvious, but perhaps of greater utility: there must be a continued sequence in the order of double elective attractions; that is, between any two acids we may place the different bases in such an order that any two salts, resulting from their union, shall always decompose each other, unless each acid be united to the base nearest to it: for example, sulfuric acid, barita, potass, soda, ammonia, strontia, magnesia, glycina, alumina, zirconia, lime, phosphoric acid. The sulfate of potass decomposes the phosphate of barita,

because the difference of the attractions of barita for the sulfuric and phosphoric acids is greater than the difference of the similar attractions of potass; and in the same manner the difference of the attractions of potass is greater than that of the attractions of soda; consequently the difference of the attractions of barita must be much greater than that of the attractions of soda, and the sulfate of soda must decompose the phosphate of barita; and in the same manner it may be shown that each base must preserve its relations of priority or posteriority to every other in the series. It is also obvious that, for similar reasons, the acids may be arranged in a continued sequence between the different bases; and when all the decompositions of a certain number of salts have been investigated, we may form two corresponding tables, one of the sequences of the bases with the acids, and another of those of the acids with the different bases; and if either or both of the tables are imperfect, their deficiencies may often be supplied, and their errors corrected, by a repeated comparison with each other.

In forming tables of this kind from the cases collected by Fourcroy, I have been obliged to reject some facts which were evidently contradictory to others, and these I have not thought it necessary to mention; a few, which are positively related, and which are only inconsistent with the principle of numerical representation, I have mentioned in notes; but many others, which have been stated as merely probable, I have omitted without any notice. In the table of simple elective attractions I have retained the usual order of the different substances; inserting again in parentheses such of them as require to be transposed, in order to avoid inconsequences in the simple attractions: I have attached to each combination marked with an asterisc the number deduced from the double decompositions, as expressive of its attractive force; and where the number is inconsistent with the corrected order of the simple elective attractions, I have also inclosed it in a parenthesis. Such an apparent inconsistency may perhaps in some cases be unavoidable, as it is possible that the different proportions of the masses concerned, in the operations of simple and compound decomposition, may sometimes cause a real difference in the comparative

magnitude of the attractive forces. Those numbers, to which no asterisc is affixed, are merely inserted by interpolation, and they can only be so far employed for determining the mutual actions of the salts to which they belong, as the results which they indicate would follow from the comparison of any other numbers, intermediate to the nearest of those, which are more correctly determined. I have not been able to obtain a sufficient number of facts relating to the metallic salts, to enable me to comprehend many of them in the tables.

It has been usual to distinguish the attractions, which produce the double decompositions of salts, into necessary and superfluous attractions; but the distinction is neither very accurate nor very important: they might be still further divided, accordingly as two, three, or the whole of the four ingredients concerned are capable of simply decomposing the salt in which they are not contained; and if two, accordingly as they are previously united or separate; such divisions would however merely tend to divert the attention from the natural operation of the joint forces concerned.

It appears to be not improbable that the attractive force of any two substances might, in many cases, be expressed by the quotient of two numbers appropriate to the substances, or rather by the excess of that quotient above unity; thus the attractive force of many of the acids for the three principal alkalies might probably be correctly represented in this manner; and where the order of attractions is different, perhaps the addition of a second, or of a second and third quotient, derived from a different series of numbers, would afford an accurate determination of the relative force of attraction, which would always be the weaker, as the two substances concerned stood nearer to each other in these orders of numbers; so that by affixing to each simple substance two, three, or at most four numbers only, its attractive powers might be expressed in the shortest and most general manner.

I have thought it necessary to make some alterations in the orthography generally adopted by chemists, not from a want of deference to their individual authority, but because it appears to me that there are certain rules of etymology which no modern

author has a right to set aside. According to the orthography universally established throughout the language, without any material exceptions, our mode of writing Greek words is always borrowed from the Romans, whose alphabet we have adopted: thus the Greek vowel Υ, when alone, is always expressed in Latin and in English by Y, and the Greek diphthong ΟΥ by U, the Romans having no such diphthong as OU or OY. The French have sometimes deviated from this rule, and if it were excusable for any, it would be for them, since their *u* and *ou* are pronounced exactly as the Υ and ΟΥ of the Greeks probably were; but we have no such excuse. Thus the French have used the term *acoustique*, which some English authors have converted into "acoustics;" our anatomists, however, speak, much more correctly, of the "acustic" nerve. Instead of glucine, we ought certainly, for a similar reason, to write glycine; or glycina, if the names of the earths are to end in *a*. Barytes, as a single Greek word, means weight, and must be pronounced bárytes; but as the name of a stone, accented on the second syllable, it must be written barites; and the pure earth may properly be called barita. Yttria I have altered to Ytria, because no Latin word begins with a Y.

Table of the Sequences of the Bases with the different Acids.

In all mixtures of the aqueous solutions of two salts, each acid remains united to the base which stands nearest to it in this table.

SULFURIC ACID.

Barita	Barita	Barita	Barita	Barita	Barita	Potass	Barita	Barita	Lead
Strontia	Strontia	Potass	Potass	Potass	Potass	Soda	Strontia	Potass	Mercury
Lime	Lime	Soda	Soda	Soda	Soda	Barita	Lime	Soda	Iron, Potass, Soda, Magnesia
(Silver?)	Potass	Ammonia	Strontia	Strontia	Strontia	Strontia	Potass	Ammonia	
(Mercury?)	Soda	Strontia	Ammonia	Ammonia(4)	Ammonia(5)	Ammonia(6)	Soda	Strontia	
Potass	(Mercury?)	Magnesia(3)	Magnesia	Magnesia(4)	Magnesia	Lime	Magnesia?	Magnesia	
Soda	(Iron?)	Glycina	Glycina	Glycina	Lime	Magnesia	Ammonia	Glycina	
Zinc, Iron, Copper	Magnesia	Alumina	Alumina	Alumina	Glycina	Glycina	Glycina	Alumina	Lead
Magnesia	Ammonia(2)	Zirconia	Zirconia	Zirconia	Alumina	Alumina	Alumina	Zirconia	Zinc, Copper
Ammonia(1)	Glycina	Lime	Lime	Lime	Zirconia	Zirconia	Zirconia	Lime?	
Glycina	Alumina(2)								
Alumina	Zirconia								
Zirconia	(Copper?)								
NITRIC	MURIATIC	PHOSPHORIC	FLUORIC	SULFUROUS	BORACIC	CARBONIC	(NITROUS)	(PHOSPHOROUS)	(ACETIC)

(1) Ammonia stands above magnesia when cold. (2) A triple salt is formed. (3) Perhaps magnesia ought to stand lower. (4) A compound salt is formed, and when hot, magnesia stands above ammonia. (5) Fourcroy says, that sulfate of strontia is decomposed by borate of ammonia. (6) With heat, ammonia stands below lime and magnesia.

NITRIC ACID.

NITRIC AND MURIATIC ACIDS.

Barita	Potass	Barita	Potass	Barita (10)	Potass
Potass	Soda	Potass	Soda	Potass	Soda
Soda	Ammonia	Soda	Ammonia	Soda	Barita (10)
Strontia	Magnesia	Ammonia	Magnesia	Ammonia	Ammonia (7,11)
Lime	Glycina	Magnesia	Glycina	Magnesia	Magnesia (7)
Magnesia (7)	Alumina	Glycina	Alumina	Glycina	Strontia
Ammonia (7)	Zirconia (8)	Alumina	Zirconia	Alumina	Lime
Glycina	Barita	Zirconia	Barita	Zirconia	Glycina
Alumina	Strontia	Strontia (9)	Strontia	Strontia	Alumina
Zirconia	Lime	Lime	Lime	Lime	Zirconia
MURIATIC	PHOSPHORIC	FLUORIC	SULFUROUS	BORACIC	CARBONIC

(7) A triple salt is formed. (8) Fourcroy says, that the muriate of zirconia decomposes the phosphates of barita and strontia. (9) According to Fourcroy's account, the fluate of strontia decomposes the muriates of ammonia, and of all the bases below it; but he says in another part of the same volume, that the fluate of strontia is an unknown salt. (10) According to Fourcroy's account of these combinations, barita should stand immediately below ammonia in both of these columns. (11) With heat, the carbonate of lime decomposes the muriate of ammonia.

PHOSPHORIC ACID.

Barita	Lime	Barita	Potass	Barita
Lime	Barita	Lime	Soda	Lime
Potass	Potass	Potass	Barita	Potass
Soda	Soda	Soda	Lime (13)	Soda
Strontia	Strontia	Strontia	Strontia	Strontia
Magnesia	Magnesia	Ammonia (12)	Ammonia	Magnesia
Ammonia	Ammonia	Magnesia	Magnesia	Glycina?
Glycina	Glycina	Glycina	Glycina	Alumina
Alumina	Alumina	Alumina	Alumina	Zirconia
Zirconia	Zirconia	Zirconia	Zirconia	
FLUORIC	SULFUROUS	BORACIC	CARBONIC	(PHOSPHOROUS)

(12) According to Fourcroy, the phosphate of ammonia decomposes the borate of magnesia. (13) Fourcroy says, that the carbonate of lime decomposes the phosphates of potass and of soda.

FLUORIC ACID.

Lime	Lime	Potass
Potass	Barita	Soda
Soda	Strontia	Lime
Magnesia	Potass	Barita
Ammonia	Soda	Strontia
Glycina	Ammonia	Ammonia (14)
Alumina	Magnesia	Magnesia
Zirconia	Glycina	Glycina
Strontia	Alumina	Alumina
Barita	Zirconia	Zirconia
SULFUROUS	BORACIC	CARBONIC

(14) According to Fourcroy, the carbonate of ammonia decomposes the fluates of barita and strontia.

Sulfurous Acid.				Boracic Acid.
Barita	Potass	Lime	Zirconia	Potass
Strontia	Soda	Strontia	Alumina	Soda
Potass	Barita (15)	Barita	Glycina	Lime
Soda	Strontia	Zirconia	Ammonia	Barita
Ammonia	Ammonia	Alumina	Magnesia	Strontia
Magnesia	Lime	Glycina	Strontia	Magnesia
Lime	Magnesia	Magnesia	Soda	Ammonia
Glycina	Glycina	Ammonia	Potass	Glycina
Alumina	Alumina	Soda	Barita	Alumina
Zirconia	Zirconia	Potass	Lime	Zirconia
Boracic	Carbonic	(Nitrous)	(Phosphorous?)	Carbonic

(15) Fourcroy says, that the sulfate of barita decomposes the carbonate of ammonia.

Table of the Sequences of the Acids with different Bases.

Barita.				Strontia.					Lime.				Potass / Soda / Magn. = Amm. / Glycina / Alumina / Zirconia — Each with every subsequent base in this order	Mag-nesia.
Sulfuric	S	C	S	S	C	S	P	S	C	P	P	P		S B
Nitric	N	S	P	N	SS	P	S	P	P	F	F	F		N C
Muriatic	M	P	SS	M	F	SS	SS	SS	F	B	B	SS		M P
Phosphoric	SS	SS	N	SS	P	F	F	F	B	SS	C	S		P F
Sulfurous	P	N	M	C	B	B	B	B	SS	S	SS	B		F SS
Fluoric	C	M	F	B	S	C	C	N	S	C	S	N		SS S
Boracic	B	F	B	F	M	N	N	M	M	N	N	M		B N
Carbonic	F	B	C	P	N	M	M	C	N	M	M	C		C M
Strontia	LM	PT	MG	LM	PT	MG	AM	GL	PT	MG	AM	GL		AM*
		SD	AM		SD			AL	SD			AL		
			GL					ZR				ZR		
			AL											
			ZR											

* The acids in this Table are denoted by their initial letters—Sulfuric acid being expressed by S, and Sulfurous by SS: Lime is denoted by LM, Potass by PT, Soda by SD, and so on. In Nicholson's Journal for March, 1809, Dr. Young gave the following technical hexameters, to aid the memory in retaining the order of these sequences, the familiar knowledge of which is important to chemists, and we may add likewise to physicians in the composition of their medicines.

Contentio aquatica; Victoria; Requies.

Rebaris*ne* *modo* *posse* ad*fore* *bellica* rosTRa?
Des *nautam* *satis* *apta* *cibo* refovere aLiMenta;
Cor *superest* *sanum*; *fla*bitque opTatus abunDe
Spiritus; has *animi* ira *feret* *tibi* acerriMA GAZas.
Ast Brontes *animosus* acerbo *fœdere* paLMas
Cæsus *fert*: ut *pro* rebus *mo*net aPTa SODales!
Si *possit*, *fato* tu*bicen* *me*Mor addat honores.
Postulat ossa relata, heu! *flebile* *condere* *mA*rMor.
Spes est *fixa*, *bonum* cœli GAZis fruiturum.
Alma huic *pax* *fiat* *orbi*, lassis *omni*POTens Des
O *pater*! Ut *flebo* jussus *canere* *armigerûM* vim!
Dire *opifex* *belli*, *cesses* *norma*M abjicere omnem.
Pax *fessos* *bona* *mulcet*, GAZis lætior auri.
præSuMAM GAZas *nempe* ad*fore* rursus *ab* alto huc;
Mira *dabit* lucra *pax*, *fortassis* *in* ultiMA Mundi.

Note by the Editor.

Magnesia.		Ammonia.		Glycina?		Alumina.	Zirconia?
Suberic?	700	Citric	603	Citric	415	410	405
(Fluoric)	620*	Lactic	601	*Phosphoric*	(648)*	(642)*	(636)*
Tartaric	618	Benzoic	599	Lactic	410	405	400
Citric	615	Sulfurous	433*	Benzoic	400	395	390
Malic?	600?	Acetic	432	Acetic	395	391	387
Lactic	575	Mucic	431	Boracic	388*	385*	382*
Benzoic	560	Boracic	430*	Sulfurous	355*	351*	347*
Acetic		Nitrous	400	Nitrous	340	336	332
Boracic	459*	Carbonic	339*	Carbonic	325*	323*	321*
Sulfurous	439*	Prussic	270	Prussic	260	258	256
(Acetic)	430						
Nitrous	410						
Carbonic	366*						
Prussic	280						

Acids.

Sulfuric.		Nitric.		Muriatic.		Phosphoric.	
Barita	1000*	Barita	849*	Barita	840*	Barita	906 *
Strontia	903*	Potass	812*	Potass	804*	Strontia	827 *
Potass	894*	Soda	804*	Soda	797*	*Lime*	(865)*
Soda	885*	Strontia	754*	Strontia	748*	Potass	801 *
Lime	868*	Lime	741*	Lime	736*	Soda	795 *
Magnesia	810*	Magnesia	732*	Ammonia	729*	*Ammonia*	(728)*
Ammonia	808*	Ammonia	731*	Magnesia	728*	Magnesia	736 *
Glycina	718*	Glycina	642*	Glycina	639*	Glycina	648 *
Itria	712	Alumina	634*	Alumina	632*	Alumina	642 *
Alumina	709*	Zirconia	626*	Zirconia	625*	Zirconia	636 *
Zirconia	700*						

Fluoric.		Oxalic.		Tartaric.	Arsenic.		Tungstic.
Lime	734 *	Lime	960	867	Lime	733⅔	Lime
Barita	706 *	Barita	950	760	Barita	733½	Barita
Strontia	703 *	Strontia	825	757	Strontia	733¼	Strontia
Magnesia	(620)*	Magnesia	820	618	Magnesia	733	Magnesia
Potass	671 *	Potass	650	616	Potass	614	Potass
Soda	666 *	Soda	645	611	Soda	609	Soda
Ammonia	613 *	Ammonia	611	609	Ammonia	607	Ammonia
Glycina	534 *	Glycina?	600	520	Glycina	580	Glycina
Alumina	529 *	Alumina	594	515	Alumina	575	Alumina
Zirconia	524 *	Zirconia?	588	510	Zirconia	570	Zirconia

Succinic.		Suberic.		Camphoric.	Citric.	
Barita	930	Barita	800	Lime	Lime	731
Lime	866	Potass	745	Potass	Barita	730
Strontia?	740	Soda	740	Soda	Strontia	618
(Magnesia)	732¼	Lime	735	Barita	Magnesia	615
Potass	612	Ammonia	720	Ammonia	Potass	610
Soda	607	Magnesia	700	Glycina?	Soda	605
Ammonia	605	Glycina?	535?	Alumina	Ammonia	603
Magnesia		Alumina	530	Zirconia?	Glycina?	415?
Glycina?	510	Zirconia?	525?	Magnesia	Alumina	410
Alumina	505				Zirconia	405
Zirconia?	500					

The comparative use of this table may be understood from an example: if we suppose that the nitrate of barita decomposes the borate of ammonia, we must place the boracic acid above the nitric, between barita and ammonia in this table, and consequently barita below ammonia, between the fluoric and boracic in the former: hence the boracic and fluoric acids must also be transposed between barita and strontia, and between barita and potass; or if we place the fluoric still higher than the boracic in the first instance, we must place barita below ammonia between the nitric and fluoric acids, where indeed it is not impossible that it ought to stand.

Numerical Table of Elective Attractions.

BARITA.		STRONTIA.		POTASS.		SODA.	LIME.	
Sulfuric acid	1000*	Sulfuric acid	903*	Sulfuric acid	894*	885*	Oxalic acid	960
Oxalic	950	Phosphoric	827*	Nitric	812*	804*	Sulfuric	868*
Succinic	930	Oxalic	825	Muriatic	804*	797*	Tartaric	867
Fluoric		Tartaric	757	Phosphoric	801*	795*	Succinic	866
Phosphoric	906*	*Fluoric*		Suberic?	745	740	Phosphoric	865*
Mucic	900	Nitric	754*	Fluoric	671*	666*	Mucic	860
Nitric	849*	Muriatic	748*	Oxalic	650	645	Nitric	741*
Muriatic	840*	(Succinic)	740	Tartaric	616	611	Muriatic	736*
Suberic	800	(Fluoric)	703*	Arsenic	614	609	Suberic	735
Citric		*Succinic*		Succinic	612	607	Fluoric	734*
Tartaric	760	Citric?	618	Citric	610	605	Arsenic	733¾
Arsenic	733½	Lactic	603	Lactic	609	604	*Lactic*	732
(Citric)	730	*Sulfurous*	527*	Benzoic	608	603	Citric	731
Lactic	729	*Acetic*		Sulfurous	488*	484*	Malic	700
(Fluoric)	706*	*Arsenic*	(733¼)	Acetic	486	482	Benzoic	590
Benzoic	597	Boracic	513*	Mucic	484	480	*Acetic*	
Acetic	594	(Acetic)	480	Boracic	482*	479*	Boracic	537*
Boracic	(515)*	Nitrous?	430	Nitrous	440	437	Sulfurous	516*
Sulfurous	592*	Carbonic	419*	Carbonic	306*	304*	(Acetic)	470
Nitrous	450			Prussic	300	298	Nitrous	425
Carbonic	420*						*Carbonic*	423*
Prussic	400						Prussic	290

MAGNESIA.		AMMONIA.		GLYCINA?		ALUMINA.	ZIRCONIA?
Oxalic Acid	820	Sulfuric acid	808*	Sulfuric acid	718*	709*	700*
Phosphoric		Nitric	731*	Nitric	642*	634*	626*
Sulfuric	810*	Muriatic	729*	Muriatic	639*	632*	625*
(Phosphoric)	736*	Phosphoric	728*	Oxalic	600	594	588
Fluoric		Suberic?	720	Arsenic	580	575	570
Arsenic	733	Fluoric	613*	Suberic?	535	530	525
Mucic	732½	Oxalic	611	Fluoric	534*	529*	524*
Succinic	732¼	Tartaric	609	Tartaric	520	515	510
Nitric	732*	Arsenic	607	Succinic	510	505	500
Muriatic	728*	Succini	605	Mucic	425	420	415

Lactic.	
Barita	729
Potass	609
Soda	604
Strontia	603
Lime	(732)
Ammonia	601
Magnesia	575
Metallic oxids	
Glycina	410
Alumina	405
Zirconia	400

Benzoic.	
White oxid of arsenic	
Potass	608
Soda	603
Ammonia	599
Barita	597
Lime	590
Magnesia	560
Glycina?	400?
Alumina	395
Zirconia?	390?

Sulfurous.	
Barita	592 *
Lime	516 *
Potass	488 *
Soda	484 *
Strontia	(527)*
Magnesia	439 *
Ammonia	433 *
Glycina	355 *
Alumina	351 *
Zirconia	347 *

Acetic.	
Barita	594
Potass	486
Soda	482
Strontia	480
Lime	476
Ammonia	432
Magnesia	430
Metallic oxids	
Glycina	395
Alumina	391
Zirconia	387•

Mucic?	
Barita	900
Lime	860
Potass	484
Soda	480
Ammonia	431
Glycina	425
Alumina	420
Zirconia	415

Boracic.	
Lime	537 *
Barita	515 *
Strontia	513 *
Magnesia	(459)*
Potass	482 *
Soda	479 *
Ammonia	430 *
Glycina	388 *
Alumina	385 *
Zirconia	382 *

Nitrous?	
Barita	450
Potass	440
Soda	437
Strontia	430
Lime	425
Magnesia	410
Ammonia	400
Glycina	340
Alumina	336
Zirconia	332

Phosphorous.
Lime
Barita
Strontia
Potass
Soda
Magnesia?
Ammonia
Glycina
Alumina
Zirconia

Carbonic.	
Barita	420 *
Strontia	419 *
Lime	(423)*
Potass?	306 *
Soda	304 *
Magnesia	(366)*
Ammonia	339 *
Glycina	325 *
Alumina	323 *
Zirconia	321 *

Prussic.	
Barita	400
Strontia	
Potass	300
Soda	298
Lime	290
Magnesia	280
Ammonia	270
Glycina?	260
Alumina?	258
Zirconia?	256

No. XXVI.

A REVIEW OF SIR HUMPHRY DAVY'S

ELEMENTS OF CHEMICAL PHILOSOPHY.

From the Quarterly Review for September, 1812.

In attempting a review of this work, we cannot avoid professing, that we are far from entertaining the impression of sitting down as competent judges, to decide on the merits or demerits of its author: on this point the public voice, not only within our own islands, but wherever science is cultivated, has already pronounced too definitive a sentence, to be weakened or confirmed by any thing that we can suggest of exception or approbation. Our humble labours, on such an occasion, must be much more analytical and historical than critical; at the same time we are too well acquainted with the author's candour, to suppress any remark which may occur to us, as tending to correction or improvement. It has most assuredly fallen to the lot of no one individual to contribute to the progress of chemical knowledge by discoveries so numerous and important as those which have been made by Sir Humphry Davy: and with regard to mere experimental investigation, we do not hesitate to rank his researches as more splendidly successful, than any which have ever before illustrated the physical sciences in any of their departments. We are aware that the Optics of Newton will immediately occur to our readers as an exception; but without attempting to convince those who may differ from us on this point, we are disposed to abide by the opinion, that for a series of well devised experiments and brilliant discoveries, the contents of Davy's Bakerian Lectures are as much superior to those of Newton's Optics, as the Principia are superior to these, or to any other

human work, for the accurate and refined application of a sublime and simple theory to the most intricate and apparently anomalous results, derived from previous observation.*

Discoveries so far outshining all that has been done in other countries, and constituting so marked an era in the history of chemistry, cannot be contemplated by any Englishman, who possesses a taste for science, without some degree of national, and even local exultation; although it is true that other individuals and other countries have contributed largely to the success of the common cause; some, by improving the principles of other departments of physics which have been so happily applied, or by furnishing the most powerful agents and the most convenient instruments, which have been employed with so much address; and others by collateral or independent speculations and researches, which have here been blended together into one system.

From all these sources our author has derived the materials of a volume, which, when compared even with the latest works of a similar nature, exhibits a more rapid and triumphant progress of improvement than can be paralleled in the annals of human invention. He has adverted, with a very laudable modesty, to the favourable circumstances under which his researches were conducted:

"Nothing tends so much," he observes, "to the advancement of knowledge as the application of a new instrument. The native intellectual powers of men in different times, are not so much the causes of the different success of their labours, as the peculiar nature of the means and artificial resources in their possession. Independent of vessels of glass, there could have been no accurate manipulations in common chemistry: the air-pump was necessary for the investigation of the properties of gaseous matter; and without the Voltaic apparatus, there

* Most persons will think this praise exaggerated. Dr. Young was not disposed to do full justice to Newton's Optics, considering that it sanctioned some errors both of experiment and reasoning which tended very seriously to retard the progress of optical discovery: such were his statements of the law of double refraction through Iceland crystal and of chromatic dispersion, and his attempted proof of the necessary diffusion of light transmitted through an aperture upon the undulatory hypothesis. It should not be forgotten, however, that it was in a great measure owing to the very merits of this great work as a model of well-devised experiments and accurate inductive reasoning that it acquired this somewhat fatal influence upon the opinions of men of science.—*Note by the Editor.*

was no possibility of examining the relations of electrical polarities to chemical attractions."

It must, however, be remembered, that almost every other discovery of importance, which has been made in science, has been facilitated by some previous steps, which have rendered practicable what might otherwise have presented insuperable obstacles to human ingenuity; nor has such a preparation ever been allowed to detract from the just applause, bestowed on those who have been distinguished from their contemporaries by a more successful exertion of talent.

Until the year 1806, Sir Humphry Davy had been remarkable for the industrious and ingenious application of those means of experiment only, which had been long known to chemists; he had acquired, at a very early period of his life, a well established celebrity among men of science throughout Europe, by the originality and accuracy of his researches; and at the same time the fluent and impressive delivery of his lectures had obtained him the most flattering marks of approbation from the public of the metropolis. But it was in the summer of that year, that in repeating some electro-chemical experiments of very doubtful authority, he was led into a new train of reasoning and investigation, which enabled him to demonstrate the important laws of the connexion between the electrical affections of bodies and their chemical powers. This was his first great discovery: and when he was complimented on the occasion by the Institute of France with the prize established by Buonaparte, it was only questioned, by those who were capable of appreciating its importance, whether they acted with strict impartiality in assigning to him the annual interest only; while he appeared to have a fair claim to the principal, which was allotted, by the donor, to the author of a discovery relating to electricity, paramount to that of Franklin or of Volta. Our author's next great step was the decomposition of the alkalis, which he effected the succeeding year: and this, though less interesting and important with regard to the fundamental theory of the science, was more brilliant and imposing, from its capability of being exhibited in a visible and tangible form. The third striking feature, which distinguishes

the system advanced in the present work, is the assertion of the existence of at least two empyreal principles; oxygen, and the elastic fluid called the oxymuriatic acid gas, being considered as possessing equal claims to the character of simple or undecompounded substances. A fourth peculiarity, which, however, is less exclusively and originally a doctrine of Sir Humphry Davy, is the theory of the simplicity of the proportions in which all bodies combine with each other; a theory respecting which hints may be found in the works of several chemists of the last century, but for the explicit illustration, and general and minute application of which, the science is principally indebted to our countryman Mr. Dalton; although the work before us tends much more to its confirmation than any other mass of evidence which has yet been collected on the subject.* On each of these four principal novelties we shall make some extracts and abstracts; having first given a hasty outline of the interesting sketch of the progress of chemistry which constitutes the introduction.

We shall not attempt to follow our author in his inquiries how far any of the Arabian physicians or magicians may be said to have been the founders of the science of chemistry, rather than the Greeks or Egyptians, or even to conjecture in what sense Firmicus, whom he has not mentioned, may have intended to employ the term chymia, which he simply introduces as a science or mystery: but contenting ourselves with enumerating the names of Roger Bacon and Basil Valentine, as the greatest chemists of the thirteenth and fifteenth centuries, and Paracelsus, Agricola, and Libavius, of the sixteenth, we shall hasten to the beginning of the seventeenth, as the true period of the commencement of the pneumatic chemistry, under the auspices of Van Helmont, who first distinctly observed the properties of several elastic fluids, which he denominated gases; and more especially of Rey, who, in the year 1630, expressly maintained the absorption of air by metals during their calcination; nor was it much later that Torricelli and Pascal began

* The account of the important labours of Berzelius upon the subject of definite proportions was translated from the German by Dr. Young, and published in successive numbers of the Philosophical Magazine from January, 1813, to April, 1814. —*Note by the Editor.*

to investigate the mechanical properties of the air with mathematical precision. About the time of the foundation of the Academy del Cimento, of the Royal Society, and the Parisian Academy of Sciences, which constitutes an era so important in the progress of human knowledge, the most distinguished chemists in Germany were Glauber, Kunckel, Brandt, Hofmann, Beccher, and Stahl; in France, Homberg, Geoffroy, and the Lemerys; and in England Boyle, Hooke, Slare, and Mayow; but with regard to the philosophical theory, especially of pneumatic chemistry, the English had advanced far beyond their neighbours, even before the publication of the correct and comprehensive speculations contained in the queries of Newton, which marked the commencement of the eighteenth century, and which may be considered as the basis of the more refined and accurate cultivation of chemical science. In pursuit of these suggestions, the order of chemical attractions appears to have been first distinctly exhibited in a tabular form by Geoffroy, about the year 1718. The idea of a single combustible principle, or phlogiston, is traced to Albertus Magnus, the contemporary of Roger Bacon, and was received from Beccher by Stahl, who advanced in support of it many ingenious experiments; for example, the decomposition of Glauber's salt by charcoal; and this doctrine was almost universally adopted throughout Europe, in preference to the more correct views of Boyle, Hooke, and Mayow. The researches of these chemists were, however, in some degree revived by the industrious Dr. Hales, although he was unfortunately misled by the idea, that all elastic fluids were essentially the same, and only distinguished by some accidental modifications, from the presence of various effluvia. The error of this opinion was clearly and elegantly displayed by Dr. Black, who published, in 1756, a little essay on magnesia and fixed air, which may be considered as the true beginning of the pneumatic chemistry. The earliest labours of Mr. Cavendish are dated in 1765, when he invented the hydropneumatic apparatus, discovered inflammable air, and made many very important experiments on the properties of gases of different kinds. Dr. Priestley followed the steps of Hales and Cavendish with the most distinguished

success, and discovered the existence of nitrous gas, nitrous oxyd, and oxygen; and exhibited, by means of the mercurial apparatus, muriatic acid, sulphurous acid, and ammonia, in a gaseous state. Macquer, Rouelle, Margraff, Pott, and above all Bergman, were in the mean time diligently pursuing their refined analyses on the continent: and Scheele was carrying on a train of investigations much resembling those of Priestley, ascertaining the composition of the atmosphere, and the properties of the fluoric and prussic acids, and the oxymuriatic acid gas. Of all these chemists, Black, Cavendish, Priestley, and Scheele were unquestionably the greatest discoverers: the facts, which they had brought forward, were in some measure systematized by Lavoisier, to whom our author thinks that 'no other inquirer except Cavendish can be compared for precision of logic, extent of view, and sagacity of induction.' Bayen, in 1774, had shown that the calx of mercury was capable of being rendered metallic, without the addition of any inflammable substance, and hence had argued against the agency of phlogiston in the revival of metals in general. In the next year, Lavoisier examined the air afforded by the calx during its reduction, which was already known to Priestley and Scheele, and called it oxygen: he demonstrated also the constitution of the carbonic acid gas, and showed that the nitrous and sulphuric acids derive their properties from the combination of their bases with oxygen: Mr. Cavendish soon after showed the true nature of the basis of the nitric acid, and made a discovery, which is perhaps of greater importance than any single fact which human ingenuity has ascertained, either before or since, that of the composition of water from oxygen and hydrogen. Soon after this, Mr. Berthollet proved that ammonia consists of hydrogen and nitrogen; and many other chemists continued a series of researches, which appeared to illustrate and confirm the doctrine of Lavoisier: the existence of phlogiston was, however, still very ably maintained by Mr. Cavendish in 1784, as the simpler of the two theories by which the phenomena might be explained; and other chemists retained the same opinion for a much longer period. In 1787, the French chemists presented to the public their new system of

nomenclature, which certainly contributed in some degree to the facility of acquiring the science, but still more to the dissemination of the doctrines of the school from which it proceeded.

"At the time (p. 53) when the antiphlogistic theory was established, electricity had little or no relation to chemistry. The grand results of Franklin, respecting the cause of lightning, had led many philosophers to conjecture, that certain chemical changes in the atmosphere might be connected with electrical phenomena; and electrical discharges had been employed by Cavendish, Priestley, and Vanmarum, for decomposing and igniting bodies; but it was not till the era of the wonderful discovery of Volta, in 1800, of a new electrical apparatus, that any great progress was made in chemical investigation by means of electrical combinations.

"By researches, the commencement of which is owing to Messrs. Nicholson and Carlisle, in 1800, which were continued by Cruickshank, Henry, Wollaston, Children, Pepys, Pfaff, Desormes, Biot, Thénard, Hisinger, and Berzelius, it appeared that various compound bodies were capable of decomposition by electricity; and experiments, which (says our author) it was my good fortune to institute, proved that several substances, which had never been separated into any other forms of matter in the common processes of experiment, were susceptible of analysis by electrical powers: in consequence of these circumstances, the fixed alkalis, and several of the earths have been shown to be metals combined with oxygen; various new agents have been furnished to chemistry, and many novel results obtained by their application, which, at the same time that they have strengthened some of the doctrines of the school of Lavoisier, have overturned others, and have proved that the generalizations of the antiphlogistic philosophers were far from having anticipated the whole progress of discovery.

"Certain bodies, which attract each other chemically, and combine when their particles have freedom of motion, when brought into contact, still preserving their aggregation, exhibit what may be called electrical polarities; and by certain combinations these polarities may be highly exalted; and in this case they become subservient to chemical decompositions; and by means of electrical arrangements, the constituent parts of bodies are separated in a uniform order, and in definite proportions. Bodies combine with a force, which in many cases is correspondent to their power of exhibiting electrical polarity by contact; and heat, or heat and light, are produced in proportion to the energy of their combination. Vivid inflammation occurs in a number of cases in which gaseous matter

is not fixed; and this phenomenon happens, in various instances, without the interference of free or combined oxygen.

"Crystals of oxalic acid," for example, (p. 159,) "touched by dry quicklime, exhibit electrical powers; and the acid is negative, the lime positive. All the acid crystals, upon which I have experimented, when touched by a plate of metal, render it positive. And in Voltaic combinations with single plates or arcs of metal, the metal is negative on the side opposed to the acid, and positive on the side or pole opposed to the alkali.

"Bodies that exhibit electrical effects previous to their chemical action on each other, lose this power during combination. Thus, if a polished plate of zinc is made to touch a surface of dry mercury, and quickly separated, it is found positively electrical, and the effect is increased by heat; but if it be so heated as to amalgamate with the surface of the mercury, it no longer exhibits any marks of electricity.—When any conducting substance, capable of combining with oxygen, has its positive electricity increased, it will attract oxygen with more energy from any imperfect conducting medium; and metallic bodies, that in their common state have no action upon water, such as silver, attract oxygen from it easily, when connected with the positive pole in the Voltaic circuit; and bodies that act upon water, such as zinc and iron, so as to decompose it slowly, refuse to attract oxygen from it, when they are negatively electrified in the Voltaic circuit.

"Acids, which are negative with respect to alkalis, metals, and earths, are separated from these bodies in the Voltaic circuit at the positive surface; and alkalis, metals, and earths are separated from acids at the negative surface: and such are the attracting powers of these surfaces, that acids are transferred through alkaline solutions, and alkalis through acid solutions, to the surfaces where they have their points of rest. It is easy to show this by making a combination of three agate cups, one containing sulphate of potassa, one weak nitric acid, and the third distilled water, and connecting them by asbestus moistened in pure water, in such a manner, that the surface of the acid is lower than the surface of the fluid in the other two cups. When two wires of platina, from a powerful Voltaic apparatus, are introduced into the two extreme cups, the solution of the salt being positively electrified, a decomposition will take place, and in a certain time a portion of potassa will be found dissolved in the cup in contact with the negative wire, though the fluid in the middle cup will still be sensibly acid."

We must here take the liberty of remarking, that several of these singular effects had been observed by Hisinger and Berzelius in Sweden a year or two before the date of Sir Humphry

Davy's discoveries; but they had neither led those chemists to entertain any suspicion of the true laws by which they are governed, nor to apply them to the production of any unknown substances. The first of the remarkable decompositions that our author effected, by means of his newly established principles, was that of potass, or the vegetable fixed alkali, from which he obtained the new metal potassium in October 1807. When a thin piece of pure or caustic potass, in its usual state of a dry hydret, or combination with water, "is placed between two discs of platina connected with the extremities of a Voltaic apparatus of 200 double plates, it will soon undergo fusion, oxygen will separate at the positive surface, and small metallic globules will appear at the negative surface, which consist of potassium." It may also be procured by heating iron filings to whiteness in a gunbarrel, and suffering melted potass to come slowly into contact with them, as MM. Gay Lussac and Thénard discovered; and even by strongly igniting potass with charcoal, as Mr. Curaudau has shown. This metal is about one seventh specifically lighter than water; it fuses at about 150° of Fahrenheit, and becomes gaseous below a red heat. It inflames violently when moistened, or when gently heated in contact with the air, affording alkaline fumes. Its powerful attraction for oxygen renders it a very useful agent in chemical analyses: naphtha seems to be almost the only substance in which it can be kept with convenience.

Soda, the mineral alkali, affords, when similarly treated, though not quite so easily, a metal much resembling potassium, but a little heavier, though still lighter than water; fusible at about 200°, and evaporating at a strong red heat: our author has very properly named it sodium; it agrees with potassium in most of its properties.

Barium was obtained in May 1808, in the form of a dark grey mass, with little lustre, by means of a process suggested by MM. Berzelius and Pontin. A portion of pure barita, moistened with water, is placed on a plate of platina, which is rendered positive by a Voltaic battery, while a globule of mercury, placed in the paste, is made negative: an amalgam is thus obtained, from which the mercury is expelled by distilla-

tion in a tube of glass free from lead, filled with the vapour of naphtha and hermetically sealed. Sir Humphry Davy had before obtained it only in combination with iron.

Strontium and calcium are procured in the same manner from strontia and lime; strontium much resembles barium, calcium is a little brighter and whiter. When the vapour of potassium is made to pass through ignited barita or lime, or some of their compounds, some potass is formed, and the earths are either partially or completely reduced to a metallic state.

Magnesium may be obtained in either of these ways, though more slowly by the electrochemical process: when the vapour of potassium is employed in a thick tube of platina, a small quantity of mercury may be afterwards introduced, which will amalgamate with the metal, and when expelled, will leave it in the form of a dark grey metallic film, not acting so rapidly on water as any other of the alkaline metals.

Aluminium, glycinium, zirconium, silicium, and itrium, have been obtained less distinctly in separate forms. Aluminium, for so we shall take the liberty of writing the word, in preference to aluminum, which has a less classical sound, withstands all attempts to decompose the earth by electrifying mercury in contact with it: but when a particle of iron is employed, with an electrical power capable of fusing it, the iron is whitened, and effervesces with water, affording a small portion of alumina. By means of the vapour of potassium also, some gray metallic particles may be obtained from ignited alumina: and glycina, similarly treated, affords a dark coloured substance, which regains the earthy appearance when heated in air, or moistened. Similar particles obtained from zirconia are found to be partly metallic, and partly of a chocolate brown colour. Silicium seems to have an appearance somewhat resembling plumbago; its alloy with iron may be obtained like that of aluminium. Itria also, treated with potassium, affords potassa, and acquires a partial appearance of metallization.

Nor have the same powerful means of analysis been less successful in demonstrating the composition of the boracic acid, from which our author has obtained a substance too little resembling a metal to be termed boracium, but which, from its

analogy to carbon, he had thought it right to distinguish by the more appropriate than elegant name boron. It is procured either by the electrical decomposition of the boracic acid, or by igniting that acid with an equal weight of potassium in a tube of iron. It is of a dark olive colour, neither fusible nor volatile in any heat to which it has been exposed; it sparkles very brilliantly when thrown into oxygen gas, and a portion of it is converted into boracic acid.

When the fluoboric acid gas is decomposed by the combustion of potassium, it affords fluate of potassa, and the boracic acid only seems to be deprived of its oxygen; but when potassium is burned in the silicated fluoric acid, there is reason to think that both the silica and the fluoric acid undergo a partial decomposition, since the gas affords a smaller quantity of fluate of lime than before the operation of the potassium: but the base of the fluoric acid has never been separately exhibited, and from the readiness with which the fluoboric gas enters into combinations, our author thinks it not altogether impossible that the fluoric acid contained in it may be a simple empyreal principle analogous to oxygen and to 'chlorine.' His opinions on the nature of these substances, which constitute the third great feature of the present work, require to be illustrated in his own words; p. 240.

"Scheele considered chlorine as an element of the muriatic acid, and hence called it dephlogisticated marine acid. By that chemist it was regarded as an undecompounded body. Lavoisier and Berthollet asserted that it was a compound of muriatic acid gas and oxygen. This idea is now universally given up; but some chemists in France and Scotland conceive that it is a compound of oxygen, and an unknown body, which they call dry muriatic acid. The weight of chlorine, its absorbability by water, its colour, and the analogy of some of its combinations to bodies known to contain oxygen, are arguments in favour of its being a compound, and it is possible that oxygen may be one of its elements, or that oxygen and chlorine are similarly constituted. I have made a number of experiments with the hopes of detecting oxygen in it, but without success; none of its compounds with inflammable bodies will afford this principle; charcoal, intensely ignited in it, undergoes no change, nor is it altered by the strongest powers of electricity. Should oxygen ever be procured from it, some other form of matter, possibly a new one, will, at the same time, be discovered as entering into its consti-

tution, and till it is decompounded, it must be regarded, according to the just logic of chemistry, as an elementary substance."

P. 237. "Chlorine and oxygen are capable of existing in combination, and they form a peculiar gaseous matter. They do not unite, when mixed together, but when existing in certain solids, they may be detached in union. To make the compound of chlorine and oxygen, hyperoxymuriate of potassa is introduced into a small retort of glass, and twice as much muriatic acid as will cover it diluted with an equal volume of water. By the application of a gentle heat, the gas is evolved, and it must be collected over mercury. I discovered this elastic substance in its pure form in January 1811, and gave to it the name of euchlorine, from its bright yellow-green colour. Its smell is not unlike that of burnt sugar. It must be collected and examined with great care, and only in small quantities at a time; a very gentle heat causes it to explode, sometimes even the heat of the hand; and its elements separate from each other with great violence, producing light. None of the metals that burn in chlorine act upon this gas at common temperatures; but when the oxygen is separated they then inflame in the chlorine. The proportion in which chlorine combines with bodies may be learned from the decomposition of euchlorine; the oxygen in which is to the chlorine as 15 to 67 in weight. If euchlorine be considered as consisting of one proportion of oxygen to one of chlorine, then 67 will be the number representing chlorine, which is most convenient as being a whole number. If euchlorine be supposed to contain two proportions of chlorine and one of oxygen, then the number representing chlorine will be 33.5. It will hereafter be shown that whichever of these data be assumed, the relations of the number will harmonize with those gained from various other combinations."

The doctrine of the simple proportions of combinations, exemplified in these numbers, which was the fourth point that we mentioned as particularly requiring to be noticed, is thus stated in the introduction.

"Experiments made by Richter and Morveau had shown that, when there is an interchange of elements between two neutral salts, there is never an excess of acid or basis; and the same law seems to apply generally to double decompositions. When one body combines with another in more than one proportion, the second proportion appears to be some multiple or divisor of the first; and this circumstance, observed and ingeniously illustrated by Mr. Dalton, led him to adopt the atomic hypothesis of chemical changes, which had been ably defended by Mr. Higgins in 1789; namely, that the chemical elements consist of certain

indestructible particles, which unite one and one, or one and two, or in some definite numbers." p. 56.

P. 117. "Mr. Berthollet, to whom the first distinct views of the relations of the force of attraction to quantity are owing, has endeavoured to prove that these relations are universal, and that elective affinities cannot strictly be said to exist. He considers the powers of bodies to combine as depending, in all cases, upon their relative attractions, and upon their acting masses, whatever these may be: and he conceives that in all cases of decomposition, in which two bodies act upon a third, that third is divided between them in proportion to their relative affinities and their quantities of matter. Were this proposition strictly correct, it is evident that there could be scarcely any definite proportions. When an alkali precipitates an earth from its solution in an acid, the earth, according to Mr. Berthollet's ideas, ought to fall down in combination with a portion of acid. But if a solution of potassa be poured into a sulphuric solution of magnesia, the precipitate produced, after being well washed, affords no indication of the presence of acid; and M. Pfaff has shown, by some very decisive experiments, that magnesia has no action upon neutral combinations of alkalis and sulphuric acid; and likewise, that the tartarous acid is entirely separated from lime, and the oxalic acid from oxyd of lead, by quantities of sulphuric acid merely sufficient to saturate the two bases; and these are distinct and simple instances of elective attraction. Again, when one metal precipitates another from an acid solution, the body that falls down is usually free from acid and oxygen: thus zinc precipitates lead and tin, and iron copper; and the whole of the oxygen and the acid is transferred from one metal to the other."

Having exhibited this outline of the general doctrines which Sir Humphry Davy has undertaken to maintain, we must pause to consider how far he seems to have left any thing further to be desired, with regard to the perfect establishment of either of them. His electrochemical discoveries, and his decomposition of the alkalis and earths, must ever remain incontestable memorials of his ingenuity and success; but on the subject of the oxymuriatic acid gas, we cannot help thinking his tone (p. 335) somewhat more decisive than the present state of the investigation altogether authorises. We do not see the absolute necessity of considering every body as simple which has never been decompounded, provided that there are strong analogical reasons for suspecting that it is really a compound. In

the present instance, there are considerable difficulties on both sides, and being much disposed to suspend our judgment until further evidence can be obtained, we must confess that a new nomenclature, founded on the adoption of the new opinion, and tending to carry with it a tacit persuasion of its truth, appears to us to be somewhat premature. Either hypothesis may be employed for explaining the phenomena; perhaps the probability is in favour of Sir H. Davy's; but the arguments, by which it is supported, cannot yet be considered as finally conclusive. We see ten or twelve different substances agreeing with the muriatic acid in a very great majority of their properties, and depending for these properties on the oxygen which they contain, and one anomalous substance only, which possesses these properties in a very slight degree, that is, sulphurated hydrogen, and which is found to contain little or no oxygen: it does not therefore appear to us to have been unphilosophical to suppose that the muriatic acid also contained oxygen. It is true that this presumption is weakened by the failure of the newly acquired powers of chemical electricity to obtain oxygen from it; but however great and wonderful those powers may be, they are not altogether irresistible, since some of the metals of the earth have been more easily exhibited by chemical than by electrical means. The oxymuriatic acid gas approaches much more nearly to the combinations of oxygen than to oxygen itself, in the facility with which it unites with metals, and in some other respects; nor do the combinations of this gas appear to resemble those of oxygen by any means so closely, as might be expected from the analogy of two elementary principles belonging to the same class. We are willing to allow, that the necessity of supposing a portion of water inseparable from the muriatic acid gas militates in some measure against the common opinion; but it must be remembered that neither the sulphuric nor the nitric acid has ever been obtained without admixture, either of water or of some other substance. On the other hand, the theory of simple proportions affords an objection of considerable weight to the doctrine advanced by our author; for the quantity of muriatic acid contained in some of the supersalts and subsalts bears a regular relation to the oxygen of the

earths or oxyds on the common supposition, and not on that of the elementary nature of chlorine; the patent yellow, for example, if we mistake not, is a substance which appears to be produced by a regular process in a constant manner, and which must, upon this hypothesis, be supposed to be a mixture of two distinct combinations, governed by no intelligible law, while, if we consider it as one of the submuriates of lead, it exhibits a strict analogy with other substances.

But even if we grant the existence of chlorine as an elementary principle, we cannot approve of distinguishing its combinations by terminations only, much less by terminations so simple as *ane*, *ana*, anée, which our author has proposed for the different combinations of chlorine with any other simple substance. According to the Linnean precept, Phil. bot. § 287, "a specific name must not be united to the generic as a termination," and Sir H. Davy has himself confessed that for calomel and corrosive sublimate the terms mercurane and mercurana would be an insufficient distinction; to say nothing of the inelegance of a French vowel in an English word, and of the difficulty of preserving the terms distinct in translations into other languages, which ought to induce us to refer all scientific nomenclature to some common form, that of the Latin language, for instance, whence they might be again derived for the use of each modern language according to its characteristic genius. We do not apprehend that the word "chlorid, following the analogy of oxyd," (p. vii,) would have been a more "theoretical expression" than the termination *ane*, and we might add to it, if necessary, dichlorid and trichlorid. In the case of the earths and alkalis, there is a manifest reason for using single words; these substances, unlike the "chlorids," occurring continually in combination, it would require great circumlocution to express the most familiar compounds, unless some such abbreviation were permitted.

With regard to the fourth principal subject on which the present work throws many new lights, that is, the simplicity of the proportions of chemical combinations, the proofs are so numerous and satisfactory, that there seems to be little room left for argument. We must say that to us, the supposed

discovery of Berthollet never carried with it any thing like conviction, and we always considered the praises and prizes which were so liberally conferred on it, as so many instances only of the facility with which the world is ready to bestow its approbation on all the performances of a person once celebrated, and frequently even the more enthusiastically the more paradoxical they appear. At the same time we must observe, that the objections of Pfaff are not so immediately applicable to Berthollet's doctrines as they appear at first sight to be; the partition of one substance between two others being principally asserted by Berthollet, as existing in the state of solution, where there is nothing to disturb it; while he considers the crystallization of one of the compounds as a new cause, perfectly capable of modifying the previous arrangement of the substances. What Sir H. Davy attributes to the experiments of Richter and Morveau was sufficiently understood by Bergman, and still more explicitly demonstrated by the contemporary or even earlier experiments of Wenzel. Kirwan's investigations on this subject were well projected, but by no means happily executed. Richter's first work on chemical combinations was published in 1792: his pompous and elaborate essays have all ended in a short and imperfect table of proportions, which has been, in a great measure, superseded by the more accurate researches of Berzelius and other late chemists. Bergman had also made experiments which prove that the oxygen, capable of enabling one metal to form a salt, was sufficient to serve for the oxydation of as much of another metal as precipitated it, and entered into combination with the acid: but it was reserved for Gay Lussac to place this law in a clear point of view, and to establish and illustrate it by decisive experiments. The principles of Berthollet were strongly and successfully opposed by Proust in 1804; he showed that in the combinations of metals with oxygen and with sulphur, certain fixed proportions are always observed in preference to others; his first experiments on the sulphurets were made in 1801. The great improvements in this doctrine, which are incontestably of very modern date, are the establishment of the simplicity of the numbers expressing the proportions of combinations, especially when

they relate to the volumes of elastic fluids, or to the comparative relations of subsalts or supersalts, and of their identity in compounds apparently of very different kinds; for example, in salts, sulphurets, and oxyds: and for these facts the science is principally indebted, after Mr. Higgins, to Dalton, Gay Lussac, Smithson, and Wollaston. The results of these principles may be most conveniently compared by exhibiting them in a tabular form; and as no table of this kind is to be found in Sir H. Davy's work, we shall here take the liberty of inserting such a one, in which we have collected most of the numbers which he has ascertained, together with some others which we have deduced from the experiments of Berzelius and Richter.

Table of the Proportional Weights of Chemical Substances entering into combination.

Substances.		Discoverers.			Weights combining.
Oxygen	—	Priestley	1774	—	15
'Chlorine'	—	Scheele	1774	—	67
Hydrogen	—	Cavendish	1766	—	1
Nitrogen	—	Rutherford	1772	—	26
Potassium	—	Davy	1807	—	75
Sodium	—	Davy	1807	—	88 or 44
Barium	—	Davy	1808	—	130
Tellurium	—	Müller	1782	—	'74' (60?)
Uranium	—	Klaproth	1789	—	77?
Chromium	—	Vauquelin	1798	—	
Antimony	—				165 (330?)
Manganesium	—	Kaim	1770	—	103
Zinc	—				66
Tin	—				110
Molybdaenum	—	Hielm	1782	—	88
Iron	—				103
Cobalt	—	Brandt	1733	—	'166' (110)
Copper	—				'120' (128?)
Arsenic	—				90
Nickel	—	Cronstedt	1751	—	'55' (110?)
Bismuth	—				134
Silver	—				205
Lead	—				398
Rhodium	—	Wollaston	1804	—	
Palladium	—	Wollaston	1803	—	'134' (106)
Mercury	—				380
Tungstenium	—	Delhuyars	1781	—	94
Gold	—				373 Berz.
Platina	—	Scheffer or Lewis	1750?	—	180 Berz.
Iridium	—	Tennant	1803	—	

Substances.		Discoverers.			Weights combining.
Osmium	—	Tennant	1803	—	
Titanium	—	Gregor?	1791	—	
Columbium	—	Hatchett?	1802	—	
Tantalium	—	Ekeberg			
Cerium	—	Hisinger and Berzelius	1804	—	86 (172?)
Strontium	—	Davy	1808	—	90
Calcium	—	Davy	1808	—	40
Magnesium	—	Davy	1808	—	38? (23?)
Glycinium	—	Davy		—	39?
Itrium	—	Davy		—	111?
Aluminium	—	Davy	1808	—	33?
Zirconium	—	Davy		—	70?
Silicium	—	Davy		—	31?
Carbon	—			—	11·4
Boron	—	Davy	1807	—	55?
Phosphorus	—	Brandt	1669	—	20 (25?)
Sulphur	—				30
Fluoric basis?	—				5·7?
Water	—	(I ox. II hydr. Cav.)		—	17
Ammonia	—	(I nitr. VI hydr. Berth.)		—	32
Potass	—	(I ox. I pot.)		—	90
Soda	—			—	'118' (59)
Barita	—	Scheele	1774	—	145
Strontia	—	Crawford	1790	—	105
Lime	—			—	55
Magnesia	—	Hofmann		—	'53' (38, B.)
Glycina	—	Vauquelin	1798	—	54
Itria	—	Gadolin	1794	—	126
Alumina	—	Margraff		—	48
Zirconia	—	Klaproth	1788	—	85
Silica	—	Margraff		—	61 (30·5)

Acids.	Weights.	Acids.	Weights.
Sulphuric	75	Tungstic	128?
Sulphurous	60	Columbic	
Phosphoric	55 Berz.	Acetic	96 Berz.
Phosphorous	35 (105?)	Formic	64 Richt.
Carbonic	41	Oxalic	{70 / 140} Berz.
Nitric	101		
Nitrous	'86' (71, B.)	Mellitic	
Muriatic	(52, B.)	Tartaric	124 Berz.
Oxymuriatic		Citric	105 Berz.
Hyperoxymuriatic		Malic	
Fluoric	21?	Mucic	
Boracic	320?	Benzoic	
Chromic		Succinic	79 Richt.
Molybdic	133	Moroxylic	110?
Molybdous	118	Camphoric	64?
Arsenic	135	Suberic	
Arsenious	120	Lactic	

By means of this table we can at once ascertain the proportions of the component parts of any salt or other compound of the substances contained in it: thus nitre consists of 90 potass and 101 nitric acid, or of 47 per cent. alkali and 53 acid in its dry state: or if we consider the white caustic potass, in the driest state in which it is exhibited by any common means, when it is still a hydret, and contains a portion of water, expressed by 17, the number for potass will become 107; and the number for the most concentrated liquid nitric acid, becoming in a similar manner 118, the proportion of alkali will be about 47½ per cent. instead of 47. And in a similar manner we find for the sulphate of barita 145 and 75, or 66 per cent. of earth, and 34 of dry acid, which is a result fully established by the most accurate analyses. It must, however, be observed, that the number here assigned to the carbonic acid is that which belongs to the alkaline subcarbonates, which are not, strictly speaking, neutral salts; and that there are some other apparent irregularities of the same nature, in the operation of the laws of simple proportions.

Besides the general doctrines which we have thus particularly examined, there are many detached passages, which we shall think it right to mention in the order of their occurrence; some on account of their novelty and interest, others because, in a work so likely to be universally studied, we wish to leave nothing unnoticed, which appears to require either correction or explanation.

In speaking of Aristotle, (p. 5,) our author seems rather to have been led away by a popular clamour, than to have studied with attention the real tenor of that great observer's mode of philosophizing. The "practice of advancing general principles, and applying them to particular instances," is so far from being "fatal to truth in all sciences," that, when those principles are advanced on sufficient grounds, it constitutes the essence of true philosophy; and Aristotle did not advance principles on physical subjects without what he thought sufficient grounds. The beauty of the theory of gravitation depends wholly on the establishment of a general principle, and its application to particular instances: and even our author appears to have applied

the general principle of simple proportions to particular instances, almost in contradiction to his own earlier researches; where, for instance, he doubts the accuracy of his experiment with diamond and potassium, because it "does not harmonize with the doctrine of definite proportions." (p. 312.) In the case of ammonia too, he has, perhaps, been partly induced by similar considerations, to repeat his former analysis, in which he "thought that a small quantity of water was found," and "very delicate experiments" having convinced him (p. 269) that no water is obtained, he has very candidly returned to Mr. Berthollet's opinion respecting the constitution of this substance.

P. 69. "For any thing we know to the contrary, gravitation and cohesion may be mere modifications of the same general power of attraction." This is a mistake not altogether uncommon with those who have not sufficiently attended to the mathematical characters of the forces concerned. Whether or no these forces may be produced by any different modifications of the same cause, we have no right even to conjecture; but their magnitude and the laws of their action are so totally dissimilar, that they cannot possibly be considered as modifications of the same power.

P. 70. There is an error in the comparative expansions of solids and fluids as here related: "100,000 parts of glass, raised from the degree of freezing to that of boiling water, became 100,083;—the expansive power of liquids in general is greater than that of solids,—100,000 parts of mercury become 101,835," that is, in bulk; but 100,000 parts of glass become in bulk 100.250, not 100,083 only; and 100,000 of zinc 100,910, its expansion being about half as much as that of mercury, instead of one-sixth, as would be inferred from our author's statement. P. 75. A "common thermometer" is not "hermetically sealed" "at the moment of the ebullition of the mercury;" for, in this case, the fluid would sink within the bulb at all common temperatures, unless the tube were much longer than usual.

P. 76. Professor Leslie has complained, in the public papers, that Sir H. Davy mentions a thermometer of Van Helmont, as similar to his differential thermometer, while, in fact, Van

Helmont's instrument was open at one end; although his explanations "incidentally involved the principle of the differential thermometer, which the author never once dreamed of reducing to use:" nor has the truth of this statement been disproved by the person who has replied on behalf of Sir H. Davy. The "principle of the differential thermometer" is too simple to be called an invention; and it is only by its ingenious application that Professor Leslie has made it an object of attention.

P. 79. A very amusing experiment, in which ether, floating on water, is made to burn, without sensibly elevating the temperature of the water one-eighth of an inch below the surface, is adduced as a proof of the great difficulty with which fluids transmit heat downwards. But it must be remembered that liquid ether is not susceptible of a temperature higher than 102°, and that a feverish hand, held at the surface of the water, would heat it just as rapidly as the boiling ether; and probably much more so, since the capacity of ether for heat is less than half of that of an aqueous fluid.

P. 80. "In solids the attractive force predominates over the repulsive; in fluids and in elastic fluids, they may be regarded as in different states of equilibrium." It is difficult to conceive how so much error and confusion could have been collected, by such an author, into so short a sentence. When one of two forces "predominates," there must be motion, and the parts of a body cannot remain at rest: indeed so far is the attractive force from predominating in a solid rather than in a liquid, that when water becomes solid, this force gives way to the repulsive, and the ice expands. Nor are the attractive and repulsive forces in any "equilibrium" in elastic fluids; the repulsive force exists here alone, and only compensated by external pressure or gravitation. It is in liquids and in solids that the attractive and repulsive forces exist in "different states of equilibrium," and probably without differing materially in degree; for the compressibility of ice appears to differ very little from that of water, and the immediate force of cohesion is intimately connected with the compressibility: but the true distinction between solids and liquids is the hardness or lateral adhesion of the one, and the perfect freedom of lateral motion possessed by

the particles of the other; and if it were necessary to assign a cause for this distinction, there is none that we could point out with greater probability, than a certain symmetry of arrangement, or an approach to crystallization, in the particles of solids, while those of fluids might be supposed to be collected together without any uniform order, and so far to be perfectly independent of each other.

P. 84. "It appears from the researches of Professor Robison, that in a vacuum all liquids boil about 145° lower than in the open air." Such an observation as this could scarcely have been made with any propriety, even before the speculations of Mr. Dalton had assisted us in forming more correct ideas on this subject. A liquid placed in a perfect vacuum might be said to boil at any temperature, however low: since the temperature at which any liquid boils is wholly dependent on the pressure to which it is subjected. We may however easily understand Professor Robison's experiments, by interpreting the term vacuum as relating to the receiver of an ordinary air-pump, not in the best repair, in which the mercurial gauge would stand at about two-thirds of an inch; for in such an atmosphere as this, both water and alcohol would in reality have their usual boiling points lowered about 145°.

P. 92. The "inaccuracy" of the thermometer must be rather increased than "counteracted," by the disparity of the expansions of fluids and solids, if it is really such as our author states it.

P. 94. With respect to heat, Sir H. Davy still professes himself an advocate of the opinion of Bacon and Newton, that it depends on a vibratory motion of the particles of bodies: but, however powerful we may allow some of his arguments to be, we cannot agree with him in thinking, that the acknowledged existence of a "motion" of expansion or contraction will go very far to prove the intimate nature of the cause of that expansion or contraction.

P. 136. The capability of thin plates, to receive a much stronger charge of electricity than thick, is here attributed to "the difficulty with which non-conductors receive polarity." Surely the explanations of Cavendish and Robison are much more luminous.

P. 141. The resemblance of the Aurora borealis to the discharge of electricity through rare air was very naturally adduced by Franklin in illustration of that phenomenon; but it cannot be admitted as a sufficient explanation, until it be shown in what manner the magnetical effects of the Aurora borealis are produced, or why its beams are always parallel to the dipping needle.

P. 149. The different powers of Voltaic batteries consisting of large and small plates, although perhaps somewhat too strongly contrasted, are very happily exemplified by experiments conducted in pursuit of Mr. Cavendish's idea of the different effect of a great quantity of electric fluid, and a great intensity of charge; and very interesting accounts are given of the operation of Mr. Children's large plates, and of the gigantic apparatus of 2000 double plates, procured by subscription for the use of the Royal Institution: but we must be contented with merely pointing out these experiments, without attempting to give a particular abstract of them.

P. 168. Sir H. Davy confirms Mr. Ehrman's discovery of unipolar bodies, which discharge the electricity of either end of the Voltaic circuit taken separately, but when connected with both, retain the character of one only; soap, for instance, remains positive, and the flame of a common taper negative.

P. 219. Note. The optical experiments here mentioned cannot certainly be sufficiently "explained on the idea of attractive poles on opposite sides of the particles of light." These experiments prove, if they prove any thing, not only "that homogeneous light, at certain equal distances in the direction of its motion, is possessed of opposite qualities, capable of neutralising each other;" but also that these qualities affect the collateral rays of any single beam in a manner precisely similar at equal distances from the radiant point: so that it would be necessary to suppose a continued stratum or film of particles to be thrown off by every luminous point, many millions of millions of times in a second, and to proceed in all directions, like an expanding shell, with an inconceivable velocity, to immeasurable distances. We do not state this as an impossibility, but as a condition necessary to be taken into consideration, without which our

author's conjecture would be wholly inapplicable to the phenomena.

P. 233. It is very justly observed that atmospheric air has not been found to differ perceptibly in its composition in the most dissimilar situations, containing always 21 of oxygen, and 79 of azote or nitrogen; that is, as it should have been added, by measure.

P. 296. A peculiar hydrophosphoric gas is described, which was discovered by the author in February, 1812; but which appears to have been previously known to Böckman and others. P. 320. Some very accurate and decisive experiments are mentioned, which seem to determine finally that the "carbureted" or carboneted hydrogens contain no oxygen, and that they exist exclusively in the forms of "carbureted hydrogen," and "super. carbureted hydrogen," or olefiant gas. P. 382. "Stannane" seems to have been known to Proust.

P. 391. It is observed that the colours on a polished surface of heated iron "cannot depend on oxydation, as they take place under mercury." But they appear between the temperatures of 430° and 580°, when the mercury has not yet boiled, and when we cannot be certain that all air has been excluded. In the case of lead, there is positive evidence that these colours are derived from the formation of litharge; and in that of iron, it appears almost impossible to doubt that they are the beginning of the scales of oxyd, which are actually thrown off, when the heat becomes more intense.

P. 435. Palladium is said not to have "been found in sufficient quantities to be applied to the purposes of the arts." But, if we are not misinformed, its ingenious discoverer, who seems to set all quantity at defiance, has furnished an auropalladium, or an alloy of this metal with gold, for the graduations of the magnificent circular instrument, which has lately been constructed by Mr. Troughton for the Royal Observatory at Greenwich; this alloy having the appearance and durability of platina, and being of a hardness better adapted for receiving the divisions.

P. 492. The "powder of Algarotti" seems to be a submuriate, and not an "oxyd" of antimony.

P. 498. The solution of potassium in hydrogen is made the basis of the explanation of the ready production of potassium by means of ignited iron filings: but we had before been told that sodium may be obtained in the same manner, and that sodium is not soluble in hydrogen; pp. 331, 335. The attraction of potass to the oxyd of iron is alleged by others, with more consistency, as a predisposing or potential affinity.

The character of Sir Humphry Davy's researches has always been that of the most interesting originality, and we have certainly no reason to complain that he has in his experiments very commonly forsaken the beaten path. But in a general work like the present, it was impossible that everything which was required should be supplied from what he had himself discovered or confirmed, and in reporting the labours of others, he has sometimes allowed inaccuracies to escape him, which a little more plodding diligence might have avoided. The processes *for obtaining the metals in purity* are often of this description: they might perhaps very properly have been omitted or deferred, as not sufficiently elementary to be read with advantage by a student: but if they were to be inserted, it would have been better to have rendered them a little more intelligible; and the entering into such an explanation of each process might often have led the author to have considered all its steps with more attention, and to have inquired if they afforded the best possible means of attaining the desired end. He seems also in many instances to have trusted too much to his memory in asserting the non-existence of certain combinations, especially those of several of the metals with carbon, and of some with sulphur.

The present volume extends only to the general laws of chemical changes, and the primary combinations of the undecompounded bodies: how many more such volumes will complete the whole of the projected Elements, it has probably not yet been possible to determine. With all its excellences, this work must be allowed to bear no inconsiderable marks of haste, and we could easily have conjectured, even if the author had not expressly told us so in his dedication, that the period employed on it "has been the happiest of his life." In that,

and in every other happiness which may have befallen him, we shall ever most sincerely rejoice, nor shall we think the public will have any reason to reproach him with having done too little for science, even if he should fail, at any future time, in his avowed resolution of pursuing it "with unabated ardour;" that he has not yet so failed, is become, from a late accident, a matter of public notoriety; and if we may expect perseverance to be at all commensurate to success, we have no reason to be apprehensive of his passing any part of his life in inactivity.

The style and manner of this work are nearly the same with those of the author's lectures delivered in the theatre of the Royal Institution; they have been much admired by some of the most competent judges of good language and good taste; and it has been remarked that Davy was born a poet, and has only become a chemist by accident. Certainly the situation, in which he was placed, induced him to cultivate an ornamented and popular style of expression and embellishment; and what was encouraged by temporary motives has become natural to him from habit. Hence have arisen a multitude of sentimental reflections, and appeals to the feelings, which many will think beauties, and some only prettinesses; nor is it necessary for us to decide in which of the two classes of readers we wish ourselves to be arranged, conceiving that in matters so indifferent to the immediate object of a work, a great latitude may be allowed to the diversity of taste and opinion.

Dr. Young also wrote notices, in the Quarterly Review, of Davy's Agricultural Chemistry and of Bancroft on Dyeing; but as they contain few observations of much originality or importance, it has not been thought necessary to reprint them.—*Note by the Editor.*

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